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The University of Chicago Science Series

THE ORIGIN OF THE EARTH

THOMAS CHROWDER CHAMBERLIN


Bruce L.
Clark

Bruce L Clar
Oct 1926

to Joe Graham.
July 6, 1945

Ruth Graham

in memory of

J. Graham

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THE ORIGIN OF THE EARTH

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THE ORIGIN OF THE EARTH

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TO
TWO AMERICANS,
AMONG PATRONS OF SCIENCE AND EDUCATION
THE MOST GENEROUS,
AMONG PHILANTHROPISTS
THE MOST WISE,
THIS LITTLE OFFSPRING OF THEIR GIFTS
IS
DEDICATED

PREFACE

In telling the story of this search for the mode by which the earth came into being, we have let the incidents that led the inquiry on from one stage to another fall in with the steps of the inquiry itself. It is in keeping with the purposes of this series of booklets that the motives which set researches a-going should have their place with the quests that arose from them. At any rate, it is clear that the reader will be at some advantage in forming his own judgment of the value of what is offered for his acceptance, if the lines along which the inquiry was approached, the conditions that affected the mental attitude of the inquirer, and the considerations that weighed in reaching conclusions are laid as frankly before him as the conclusions themselves.

The reader will of course choose his own pace, but it were well if it were deliberate. Pictures of nebulae and nebulous pictures rise slowly into good definition. Besides, the interpretations we offer are tentative; it were well to detain them a little for scrutiny as they pass under review. The final story of the birth of the earth will come only after a time when the vestiges of creation have been more keenly discerned and more faithfully rendered than is possible now.

And yet one may indulge the belief that, in due time and with adequate patience, the earliest history of the earth may be deciphered with a confidence not unlike that we now repose in the interpretation of its stratigraphic pages.

The reader will appreciate the kindness of the Directors of the Lick, the Yerkes, and the Mount Wilson observatories in permitting free use of their photographic material for illustrations. I am under great obligations to Professors F. R. Moulton, H. N. McCoy, and R. T. Chamberlin for reading my manuscript, in whole or in part, and for critical suggestions that have been very helpful. Throughout the whole of the cosmogonic studies, ranging backward through two decades, of which this little booklet is the outcome, I have been a debtor in a peculiar degree to Dr. Moulton for aid, advice, and co-operation in various inquiries involving celestial mechanics and kindred applications of the higher resources of mathematics. This assistance has been nothing less than indispensable.

T. C. C.

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INTRODUCTION

If it shall seem strange to anyone that a student of the story of the rocks should turn aside from a field so solid and congenial to venture wantonly into the nebulous wilds of cosmogony, I can only plead in defense the urgent necessities of the scientific chase. It came to be clear that only by close pursuit along the trail that led into the cosmogonic fens and fogs was there any hope of overhauling the quarry that had awakened my instincts of pursuit—a pack of sophistical sprites that had long been wont to vex a pet climatic enigma on whose solution I had set fond hopes. It may be some little further extenuation of my temerity to plead that, at the outset, the trail was picked up and the chase begun almost as far away as possible from the pass that led into the bogs and mists, and that at the start the trail was as cold as a glacier.

My early geologic work happened to fall in a tract that was overlain by a thick mantle of glacial drift and underlain by the sheeted sediments of the Paleozoic seas. Above, there was little but the products of the strange ice invasion; below, there was little within reach but the products of the ancient seas. Coral reefs and crinoid fields contested with moraines and drumlins the place of first affection. Early bias favored the sea life, but the glacial beds were uppermost in the field and soon came to be foremost in the zest of inquiry. How ice sheets could have crept so far south upon the low plains in the heart of our continent grew to be a more and more insistent puzzle as the verity of the invasion grew more and more incontestable. There were indeed inherited theories, but when these were brought to test by the precise realities of the record as it was met from day to day in the field, they seemed to limp under the burden of explanation they had taken upon themselves, and so, one after another, they were turned out to pasture as lame horses no longer fit to be ridden. New theories were sought in their place and ridden as far as they would go. Among the rest, an old suggestion of Tyndall's was saddled up and mounted with little thought of the outcome, and this proved to be the mount that was to carry me into the fens and fogs of cosmogony.

Tyndall had found, in the course of his physical researches, that carbon dioxide was an efficient absorber of heat, and so he had entertained the suggestion that a deficiency of this gas in the atmosphere might be the cause of the low temperatures that gave rise to the ice sheets. The suggestion had been made so long before my day that it had been well-nigh forgotten. The probabilities seemed all against its tenability. Tyndall had neglected to point out any natural process by which such a former deficiency in carbon dioxide could have arisen, and had thus left the theory without a working basis; still, as a physicist, merely throwing out a suggestion incidental to the main line of his study he had done all that could fairly be required of him. The history of the atmosphere, as then currently interpreted, looked quite the other way. It was generally held, in accordance with Laplace's beautiful theory of the origin of the solar system, that the earth was at first all gas—all atmosphere as Laplace put it—and that all the carbon later locked up in the coals, the oils, the carbonaceous shales, the limestones, and the other carbonates was then free gas and diffused throughout this great globe of gas. It was held that later, when cooling had made some progress, the refractory matter that was soon to form the rocks gathered into a white hot globe of lava, but that still all the oxides of carbon and all the water remained in the hot atmosphere and gave it enormous extent and density. It was reasoned that later, as cooling proceeded, the waters must have been gathered gradually to the earth, but that the carbon dioxide still persisted in the atmosphere until slowly, as the ages went on, it entered into union with the rock crust to form carbonates, or was extracted from the air by plants to form coals and other carbonaceous deposits. And so, each earlier age was thought to have held more carbon dioxide in its atmosphere than the succeeding ones. If this were true, it seemed idle to go backward in time to find deficiency in carbon dioxide.

Besides this infelicity, there seemed to be in this very fact of a great decline from the very hot to the cold a basis for a plausible hypothesis of glaciation—the simple, natural trend of a moribund earth toward a cold senility. The sun was growing cooler and less heat came to the earth; the earth-body was growing colder and was shrinking and cracking and drinking in the water on its surface; the carbon dioxide, oxygen, and other elements of the air were being drawn in also and were uniting with the rocks, and so they blanketed the earth less and less effectually; less moisture rose into this thinned cold atmosphere and so there was less blanketing by vapor, and even when it rose, the vapor was more promptly condensed to cloud or floating frost, and in this form cut off and reflected away the sunlight. So it was said that the earth was cooling off and drying up, that glaciers and deserts were increasing, and that a final desiccation and a final winter were coming events of the near geologic future. We were told how the moon had lost its seas (behold the *Mare!*) and how its atmosphere had been absorbed; and then the moon was brought into court as a witness to the impending fate of the earth. Our recent icy stage was but an October frost; December was yet to come. Such was the picture, and, granting the cosmogonic views then current, such was the logical drama foreshadowed by the earth's great decline from a hot beginning toward a cold end. "The final winter," "the universal desert," "the last man," were moving themes, and there was much fine writing—albeit of a gruesome sort—by those who delight in such things.

But this theory of a simple decline from a fiery origin to a frigid end, from a thick blanket of warm air to a thin sheet of cold nitrogen, consonant with the current cosmogony as it was, logical under the premises postulated, pessimistically attractive in its gruesome forecast, already in possession of the stage, with a good prospect of holding it—this theory of a stupendous descensus none the less encountered some ugly facts as inquiry went on. It seemed to accord well enough with an ice age, *if* the ice age came *only* in the later stages of the earth's history, but it was ill suited to explain an ice age in the earlier geologic eras. Unfortunately for it, there began to appear signs of ice ages far back in time, and, besides, some of these had their seats much nearer the equator and, in other respects, were even stranger than the latest great glaciation. The evidence of these earlier and stranger glaciations was at first quite naturally received with incredulity, but the proof grew steadily stronger with every new test, and the range of the evidence was found wider and clearer as exploration advanced. While all this should have weakened, and did weaken, the fundamental concept of great warmth and a rich atmosphere in the earlier ages, while it should have roused skepticism as to the verity of the cosmogony on which it was based, and perhaps did so, still the old thermal concept and the old cosmogony continued to hamper all attempts at a radical revision of glacial theories. The old ideas served as a handicap to the suggestion of Tyndall perhaps more than most other attempts at an explanation of an ice age.

None the less, it seemed to me worth while to inquire what might be the possible climatic effects of secular variations in the constituents of the atmosphere, not merely such changes in the carbon dioxide as Tyndall had suggested, but whatever changes had taken place in any of the constituents, not the least among these the variations in water-vapor, the factor that comes and goes with a peculiar freedom of its own. Back of this search for variations, it was of course important to inquire what agencies could cause such variations.

It was not long before a plausible reason for variation in carbon dioxide was found. In a study of the deformations of the crust of the earth, attention was soon centered on the evidence that stresses had arisen within the body of the earth as time went on and had gathered in force so long as the crust had been able to withstand them, but that, when resistance was no longer possible, the crust had yielded, and had become crumpled and folded into mountains, or bowed up into great swells, or warped up into plateaus. There was naturally much riving, fissuring, and crushing of the rock in the course of these processes. Back of these there seemed also to be grander movements by which areas of continental magnitude were lifted, while areas of oceanic extent were depressed. The waters were thus drawn more deeply into the great basins while the continents stood more boldly forth. In these various ways, wider and fresher contacts of the air with the rocks arose after each of these episodes of readjustment and the active constituents of the air entered into combination with the rocks at accelerated rates.

But when the stresses of the crust had been eased by these episodes of warping, folding, and faulting, a long epoch of crustal quiet ensued awaiting another such growth of stresses into strength enough to force a new episode of disruption. During such long epochs of quiescence, the rugosities of the surface were worn down by the elements, in a greater or less measure, and the débris was carried into the oceans where it displaced an equal volume of water and, by so much, lifted the sea-level. So, too, all this time the sea was gnawing steadily at the borders of the land and creeping out upon it. In doing this, it was aided by the lifting of the breaker zone—its cutting edge—by the deposit of sediments on its bottom. A study of the stratigraphic record showed that, at times, a third or a half of the continental platforms were covered by the overlapping of the sea and the action of the air upon the rocks was thus shut off. At the same time, the lands that were not thus covered by the sea had been brought low, in some large measure, by erosion, and became covered by a deep mantle of soil and residual clay, and hence suffered a notably lessened effect from the action of the air. Such epochs of base leveling were therefore clearly times of very slow depletion of the atmosphere.

Here then was a natural process of a large order by virtue of which the air was robbed of its active elements in one set of stages at a relatively fast rate, while in the other set of stages at only a relatively slow rate. The cause of the fast action was, if a technical term may be pardoned, diastrophism; the cause of the slow action was planation. Each stage occupied a long time, but the periods of planation were much longer than the episodes of diastrophism.

The recognition of this alternation of rapid atmospheric depletion with slow atmospheric depletion gave a pulsatory aspect to the atmospheric history. When, in addition to this, it was recognized that the earth through its volcanoes had all along been *feeding* the atmosphere as well as feeding upon it, and that this feeding was also pulsatory, the case took on troublesome complications, and a more severe scrutiny of the stratigraphic record, and of the relics of life imbedded in it, became imperative. In the course of this, still further departures from the generalizations of the inherited view came to notice. Desiccation products were found to be scarcely less abundant and characteristic in the early strata than in the later, and no steady progress from humidity to aridity seemed to mark the progress of time; nor were there found any conclusive evidences of even an oscillatory progress from predominant humidity to predominant aridity. If the record favored any generalization, it seemed to be that the severest and most prevalent period of aridity fell near the middle of the stratigraphic record.

When the testimony of life was similarly rescrutinized, with as much freedom from inherited presumptions as possible, it failed to show clear evidence that the early atmosphere was in any essential respect different from the atmosphere of the later ages, particularly when the units of comparison embraced an adequate lapse of time to cover the cycles of variation. Even when the inquiry was pushed back to the very earliest strata that carried a good record of the life of the times, not only was the inherited view found wanting in clear support, but adverse evidence accumulated rather than disappeared.

When the inquiry was pressed still farther back, and support for the postulate of a molten globe was sought in the crust itself, it was not forthcoming. With strange perversity the supposed granite *foundations* proved to be granitic *intrusions*. Thus in a literal sense the very foundations of the old view proved illusive.

It was thus that the trail was followed back, with a weakening faith in the inherited theory, till the borders of the primitive stage were reached. But one further step remained—to examine the cosmogonic postulates themselves. Could the earth ever have had the vast hot atmosphere postulated? Was the earth's gravity sufficient to hold so vast and vaporous an envelope at such high temperatures and in such an intense state of molecular activity as the old mode of genesis assigned? Was the gaseo-molten genesis a reality? Thus I was already across the pass that leads from the land of rocks into the realm of cosmogonic bogs and fens. Its mists were already gathering over the path ahead. Strangely enough, the cold trail of the ice invasion had led by this long and devious path into the nebulous field of genesis.

CHAPTER I

THE GASEOUS THEORY OF EARTH-GENESIS IN THE LIGHT OF THE KINETIC THEORY OF GASES

THE OLD VIEW OF GASES

At the time Laplace put forth his hypothesis of the origin of the solar system, the true nature of gases was unknown. Just what view of their nature was held by him we are not warranted in stating with confidence; not unlikely it was the view that was current during the early half of the last century, which regarded the molecule as the nucleus of concentric spheres of alternating attraction and repulsion. At appreciable distances, the outer spheres of attraction prevailed, and the molecules were drawn together. When the molecules were forced somewhat closer together, the spheres of repulsion came into play and there was resistance to compression and a tendency to expansion. When, however, the molecules were brought into extremely close relations by cold or by compression, the inner spheres of attraction were brought into play, and the molecules came still closer together to form liquids and solids. If such a view was entertained by Laplace, it was not unnatural for him to suppose that gases might gather about a planet to any extent, or gather about bodies much smaller than planets, or even gather together without any solid nucleus at all. And so the gaseous theory of the origin of the earth may have been quite consistent with the views of the behavior of gases current when it was given to the world, however much it may be found open to criticism from a new point of view arising from a new theory of gases.

While this defense is due the author of the nebular hypothesis and his earlier followers, it is none the less fair to put an old gaseous theory of earth genesis to the test of a new theory of gases. The Laplacian hypothesis of the origin of the earth is eminently a gaseous theory. It is indeed entitled to be regarded as the type of gaseous cosmogonic theories. It has the merit of being specific and systematic beyond most of its class. It is brought definitely home to the special case of the earth. No other of its class has these eminent merits in equal degree. It will therefore be treated in this discussion as the representative of its class, and scant space will be given to those cruder theories that are not clear and specific in their application to the origin of the earth, for the earth is the one celestial body of which we have a sufficiently intimate knowledge to make it susceptible of satisfactory use as a test of theories of planetary origin.

THE KINETIC VIEW OF GASES

Since the great French astronomer and mathematician framed his hypothesis—a little more than a century ago—much has been learned of the molecule and of its mode of action, especially its mode of assembling to form gases. Much more is yet to be learned no doubt, but a real start has been made. With marvelous skill, physicists have begun to deal with single electrons and alpha-particles and to toy with single ions and atoms of electricity. It may almost be said that a personal diagnosis of a molecule is now in order; as also a detailed story of its mode of joining its neighbors in assemblages to form a gas, and of its behavior toward its neighbors; for a molecule is not a gas but merely one of many of its kind to make a gas. Gases are congregations; an atmosphere is a great congregation drawn together by a commanding attraction.

Now it is quite certain that a molecule is not a little, round, hard, uncuttable thing surrounded by concentric layers of attraction and repulsion, as once imagined, nor is it even a group of little, round uncuttables so surrounded. It cannot be piled up indefinitely in the easy way once so conveniently taken for granted—unless there is an adequate commanding force. It is already known that the molecule is a very active little body, a fidgety midget, always apparently in a whirl or a quiver. So true is this that two molecules could scarcely be brought together, however gently, in such a way that they would rest quietly side by side. The whirl or the quiver of one or the other, or of both, would be almost sure to send them apart with a sharp recoil (unless there is a special attraction which brings on chemical union; but this lies outside the present problem). In our atmosphere, the gravity of the earth is always pulling the molecules together. If, in the course of their collisions and recoils, a start toward a vacuum is made, a crowd of molecules from all sides is pulled or pushed into it by gravity, and there is a little crush and a new set of recoils springs from this. In some such ways as this, it comes about that the molecules of an atmosphere are perpetually flying to and fro, colliding and rebounding, and these collisions and rebounds follow one another with extraordinary frequency. The late Clerk Maxwell, a mathematician and physicist of singular acuteness of insight, computed that the number of collisions and rebounds that commonly take place in the air about us, under ordinary conditions, is several billions per second. The frequency is greater for the molecules of low specific weights than for those of higher specific weights, and the speeds of the lighter molecules are the greater.

The collisions and recoils also increase in number and vigor with rise of temperature. It is to our purpose to make special note of the fact that the velocities of the molecules in their recoils increase both with the temperature and with the lowness of their specific weights. Light molecules are likely to be swift molecules.

PLANETARY CONTROL OF GASES

Now in the case in which we are specially interested, the gaseous envelope of a planet, the assembling power—and the holding power—is the gravity of the planet. It thus becomes a vital question how large a congregation of molecules a given planet can draw together and how well it can hold them. The chief difficulty in retaining the entire assemblage, for any length of time, lies in the extreme activity of the molecules, and in the swiftness of their rebounds as they spring back from collisions with one another. The molecules are very elastic; if they are not perfectly elastic, as commonly assumed, they are immeasurably near it. So, when they collide, there is a new set of velocities, and the nature of the new set depends in part on the way in which they strike one another and the way in which they rebound, in part on the temperature, and in part on other features of the particular case, including the rotations and vibrations that happen to be affecting the molecules. So, also, there arises a new set of directions of motion, and this also depends on the way the collisions take place and on the other factors of the case. The collisions are so many and the results so various that they cannot be dealt with individually but only by grand averages and by the laws of probability. It is greatly to our purpose, however, to know that some molecules gain in velocity, while others lose; that each new collision almost certainly raises or lowers the individual speeds of the molecules involved. And so it happens that, by virtue of the chances that arise in a great series of collisions, some one molecule may be given higher and higher speeds in succession up to any calculable amount; while some other molecule, by way of offset, may fall lower and lower in velocity until it is stopped altogether. In such a case, of course, it remains still only until it is hit again and set off in a new series of experiences. The directions which the molecules take in rebounding are various to an indefinite degree and practically cover all directions. These perpetual changes of speed and of direction affect all the molecules drawn into an atmosphere about a planet.

As already remarked, these velocities cannot be dealt with individually because of their immense numbers and the rapidity of their changes; but Clerk Maxwell, Boltzmann, and others have shown that the partition and distribution of energies and of velocities are expressed by the law of probabilities, a law which holds with great fidelity when prodigious numbers of events of a kind are involved; and this is eminently the fact in this case.

At all times, according to this law, the speeds of certain proportions of the molecules will rise above the mean velocity to given higher and higher velocities up to a theoretically infinite speed for a vanishing number, or, on the other hand, will fall lower and lower down to zero speed.

Now this is quite to the purpose of the test we wish to make, for we may learn from this law how often a given molecule will acquire a speed sufficient to enable it to get away from the control of the planet, if other conditions are favorable. Under the law of probabilities, any molecule of a planet's atmosphere may, in infinite time, acquire a velocity high enough to escape in spite of the planet's gravity, if other conditions do not prevent. We must therefore look well to these other conditions, for much depends upon them.

THE ERRONEOUS AND THE TRUE CRITICAL VELOCITY

Now let us look, for a moment, at the other side of the equation, the holding power of the planet. If it were possible for the planet to be alone in space, its gravity could overcome the motion of a molecule moving away from it in all cases in which the speed of the molecule is less than the velocity it would acquire if it fell from an infinite distance to the planet, free from interferences of all kinds. If the molecule has this "velocity from infinity," or a higher velocity, it will go out to infinity, in theory; in reality, it will continue to go away indefinitely but slower and slower because of the backward pull of the planet's gravity, which, however, grows feebler and feebler as the molecule gets farther and farther off; the attraction of the planet, however, will never overcome the molecule's flight entirely. The limit of a planet's control over free molecules under these ideal conditions is then this "velocity from infinity."

The same fact may be put in another way, and that gives another picture, and another name. If a molecule is shot out from the earth on an unobstructed path with a velocity such that the path becomes an ellipse, the gravity of the earth will bring it back; but if it is shot out with a velocity such that its path becomes a parabola, it will not return. And so the limit of the planet's power of control is known as the "parabolic velocity." The "parabolic velocity" is the same as "the velocity from infinity," in the case of any planet. A still higher velocity gives a hyperbolic path. A velocity sufficient to give a molecule a parabolic path or a hyperbolic path is beyond the planet's control even when the planet is quite alone in space and there are no other planets, or suns, or other bodies, to help it escape by attraction counter to that of the planet.*

For the surface of the earth the "parabolic velocity," or "velocity from infinity," is about 11.2 kilometers, or 6.9 miles, per second. It follows that if a molecule acquires a velocity of this magnitude, and is directed away from the earth, and has a free path, it will escape

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*A misleading impression has gained some currency from a lack of precision of statement in certain treatises. In military science, it is customary to compute the flight of projectiles as though their paths were parabolas. This is a convenient approximation, but is not rigorously accurate, though often quoted as though it stood for the strict law of all such projectiles. When men go out to shoot at one another, convenience in computation may be indulged and some latitude in language is quite sure to come into use, but it is not proper to transfer either the indulgence, the language, or the purposes they serve, to the celestial realm.

from the earth's control. It would do so even if the earth were quite alone in space and there were no rival bodies trying to pull the molecule away. This velocity has commonly been taken as the "critical velocity" of escape—"critical" because it was thought that all velocities below this could be controlled by the earth, while all above it could not be. But this notion needs rectification. For the imaginary case of a planet completely isolated in space, it holds good. But no planet is isolated in space; it could not be so isolated and be a planet. There must be a sun where there is a planet, and in our system there is a family of planets and satellites. Now if a molecule is shot away from the earth toward Jupiter, that giant planet pulls the molecule toward it, while the earth pulls the molecule backward. The degree of influence felt by the molecule is the difference between the pull of the earth, near at hand, and the pull of Jupiter, far off. The Jovian effect, however, because of distance, is minute and negligible. But when the great attraction of the sun is brought into competition with that of the earth, as it necessarily is at all times, the case takes on a new aspect. It is clear, even to the layman, that a molecule can go no great fraction of the distance to the sun before the attraction of that great body on it will be superior to that of the earth. As a matter of fact, there is only a comparatively small space about our planet within which its gravity is even differentially greater than that of the sun, for the sun rules the whole space of the solar system in a lordly way. There is merely reserved a small spheroid about each of the planets that is under their own control primarily; even over this the sun holds indirect sway by reason of its control over the planets themselves. The space about the earth within which its gravity is sufficient to draw a body to it against the direct attraction of the sun is its "sphere of influence," or, in terms better suited to our purposes, its "sphere of control." If a molecule is placed outside this space, with only the common motion of the solar system, it will be directly controlled by the sun; if within this space, it will be directly controlled by the earth.

This "sphere of control" is not a true sphere, but rather a spheroid of three unequal axes. The minimum radius, according to Dr. F. R. Moulton, is about 1,000,000 kilometers, or 620,000 miles; the maximum radius, about 1,500,000 kilometers, or 930,000 miles. The dimensions vary as the earth approaches or recedes from the sun, but this is immaterial to our purpose except as showing that the earth's sphere of control is not a fixed feature; it is rather a creature of circumstances.

Now, the true "critical velocity" of escape is the velocity acquired by a fall, not from an infinite distance, but from this limit of the sphere of control, about 1,000,000 kilometers, or at most, 1,500,000 kilometers from the center of the earth. The difference between a fall from infinity and a fall from 1,000,000 kilometers seems something enormous, but the difference in the velocities acquired is by no means so serious as it might seem, for by far the larger part of the velocity of a body falling toward the earth is gained in the lower portion of the fall. In numerical value the rectification required is more nearly like that of replacing a meter measure by a yardstick. But the rectification, though not large numerically, is radical in importance, as we shall see later, for it affects the mode of escape of molecules as well as the velocity of escape.

THE ULTRA-ATMOSPHERES

We must now go aloft, for the whole issue lies there, the modes of escape and all. It is of little moment how great a velocity a molecule may acquire in the lower air, or how often it acquires it; it cannot escape, for it is closely surrounded on all sides by an atmosphere that acts as a barrier. It can only plunge into the multitude of molecules that crowd about it, and in so doing dissipate its energy and damp its velocity. It is only in the extremely thin air of the higher regions that a molecule can find a clear path by which to escape. Let us then go aloft and see, as well as we may, the state of things there; let us go up by steps, not only to the heights usually set as the limits of the atmosphere, but all the way up to the limit of the sphere of control.

In the lower levels, the paths of the molecules between collisions are extremely short and hence straight, for the gravity of the earth can bend their paths in only an infinitesimal way in the small fraction of a second occupied in their flights. But higher up, where the molecules are sparser, the paths between collisions become longer and slight curvatures begin to appear.

Still farther up, where the molecules are widely scattered, the curvatures grow more pronounced. When the scattered condition becomes still greater, the earth's gravity may stop the outward flight of some molecules that have rebounded outward before a collision takes place, and this gravity may cause them to turn back on elliptical curves toward the earth. When, with still further ascent, the air grows attenuated enough, these outward flights and returns without collision come to be the dominant feature.

THE KRENAL ATMOSPHERE

Thus far in the ascent, we have followed mainly in the footsteps of Dr. Johnstone Stoney,[2] the pioneer in this field, save that he did not recognize the sphere of control and the qualifications which it imposes. By an analysis of the case he saw that this "fountain-like" action must become the prevalent one when a sufficient degree of atmospheric rarity is reached, and directed attention to the fountain-like character which must thus be assumed by the outer atmosphere. As we may find the term "fountain-like" cumbersome for the frequent use we shall want to make of it, perhaps we may use *krenal* in its stead, for this means the same. It will serve our convenience to distinguish this upper region as the *krenal* zone, and the lower region as the *collisional* zone, since the latter is collisional in a singularly intense degree.

In the picture of the atmosphere delineated by Stoney, the zone of fountain-like or krenal loops served as the outer atmospheric border. The picture is not without its beauty, the whole summit of the atmosphere a mass of vaulting molecules, describing loops of multitudinous forms and dimensions set in all possible directions.

Under the law of probabilities applied to molecular velocities, some of these vaults must reach the limit of the sphere of control, some must go beyond, but the greater multitude must fall short in various degrees. Those molecules that leap beyond the limit of the earth's control enter the sun's sphere of control, where they pass into orbits about the sun and are lost to the earth's atmosphere. This is the first and simplest mode of escape, the single leap, the only mode commonly recognized in the past, but in reality not the only mode.

The krenal zone of the atmosphere is thus pictured as reaching from the collisional atmosphere outward to the limit of the sphere of control. The molecules in this zone are highly dispersed in its lower parts and they grow more and more scattered toward the outer limit. It is wise to emphasize the extremely scattered state of the krenal molecules, especially in the outermost zone, but it is an error to ignore their existence or importance. Because the krenal atmosphere is so much more attenuated than the lower atmosphere, it will be conservative to call it an ultra-atmosphere.

THE ORBITAL ATMOSPHERE

But we may, however, not complete our picture of the atmosphere, as Dr. Stoney seems to have done, with a beautiful mantle of krenal loops enveloping the collisional atmosphere. The vaulting molecules are liable to collision at any point in the course of these krenal loops, and, under the law of probabilities, such collisions are quite sure to occur. The rebounding molecules may strike in any direction, as in other cases of collision, and there may be all the varieties of partitions of energy and of redistributions of motion that take place between colliding molecules in other cases. It follows that a certain percentage of the rebounding molecules will move more or less parallel to the earth's surface, and a certain proportion of them will, under the laws of partition of motion, have velocities high enough to carry them into orbits about the earth. In proportion as the new courses approach parallelism to the earth's surface, the molecules will escape the denser atmosphere, and will continue to circle about the earth indefinitely, except that sooner or later they are likely to meet some molecule of their own class in an orbit that happens to cross their own, or some vaulting molecule in the course of its krenal flight. But for these contingencies, they would have reached a condition of stable motion, since orbits are admirable types of stability and perpetuity. It is a really curious transition for a molecule to pass by a series of gradations from such an extremely jostled state as prevails in the lower atmosphere, where it was suffering several billion diversions or reversals of motion per second, into a steady orbital motion in which it follows an orderly curving path for an indefinite period. These orbital molecules thus form a quite marked class, and constitute a third element of the earth's atmosphere, an additional ultra-atmosphere—the orbital atmosphere.

To unify the new picture, let us reflect that on logical analysis it appears clear that atmospheric molecules are actuated by three quite distinct modes of action, though derivable the one from the other—the collisional, the krenal, and the orbital. Designating the molecules of each class as an atmosphere, they constitute (1) the common or collisional atmosphere, (2) the krenal atmosphere or ultra-atmosphere, and (3) the orbital atmosphere or ultra-atmosphere.

The paths of the orbital molecules lie solely in the outer portion of the sphere of control. They cannot be perpetuated or even developed in the collisional zone because of the interference of other molecules, nor can orbital paths be maintained for any appreciable length of time in the denser part of the krenal zone, for lack of sufficiently long free paths. They can persist only in the extremely attenuated portion of the krenal zone where the contingencies of collision are relatively small. Molecules in orbital flights and in krenal flights thus cross and recross, in their very open fashion, a common area, the outer part of the sphere of control, which neither, nor both, can be said to fill, but only sparsely to traverse. In this common area these two kinds of flights cross one another, the krenal directed chiefly outward and inward, the orbital more or less laterally and curvingly. The law of probabilities implies that a certain number of collisions will take place between the vaulting molecules and the orbital molecules. So, too, since the orbital molecules circle round the earth in various directions in orbits of various dimensions and configurations, though all elliptical, it follows that a certain proportion of these will also collide with one another.

THE PROGRESSIONAL MODE OF ESCAPE

Now when such collisions take place, the molecules in orbits may either be driven earthward into smaller orbits—which may not unlikely strike into the collisional atmosphere—or else be driven outward into larger orbits. One or another of these results is almost certain to follow any collision that may happen to molecules in the course of their orbital flights, whether they encounter one of their own class or a molecule in krenal flight. Thus it appears that krenal molecules, as an incident of their vaulting flights, may not only drive molecules into orbital flights, but may change these flights into larger or smaller orbits, and, under the law of probabilities, will inevitably do both these things in a certain percentage of cases. Now, it is obvious that in the course of such changes of orbit, the limit of the sphere of control will be passed in a certain percentage of cases, and the molecule will escape from the ultra-atmosphere.

Here then is a second mode of molecular escape—escape by a series of orbital changes. In the single mode of escape heretofore commonly recognized, the molecules must vault by single leaps from points in the collisional atmosphere into space beyond the limit of control. They must then have at least the "critical velocity." In the mode of escape just set forth, the molecules pass from the collisional atmosphere outward step by step. The accession of velocity at any one time need be little more than what is requisite to give the molecules revolutionary courses about the earth.

For a circular orbit the velocity of revolution is to the parabolic velocity as $1:\sqrt{2}$, or $1:1.4+$. The velocity necessary for escape is notably less than the parabolic velocity as already pointed out.

The recognition of this second mode of escape makes necessary a second rather large correction in conclusions adverse to atmospheric escape that, in spite of the fact that they were based on the erroneous assumption that the parabolic velocity is the critical velocity and that escape takes place only by single leaps from the imagined surface of the collisional atmosphere, have had currency. Escape by the progressive orbital method does not require more than about five-sevenths of the velocity requisite for escape by a single leap, and that may be acquired by instalments without necessary loss of preceding increments. There may be many steps in every escape; and indeed there may be many backward steps mingled with the forward steps. So, too, the time that elapses between the successive steps may be long or short to indefinite degrees, for the interval depends merely on the contingency of the next collision which is indeterminate. A molecule pursuing an orbit continues its revolutions indefinitely until a collision arises to make it do otherwise. It follows that the total time taken in effecting an escape is indeterminate, and may be long. This gives a distinctly new aspect to the whole problem of molecular escape from the atmosphere.

ATMOSPHERIC INTERCHANGES AND EQUILIBRIA

But we are not quite at the end of the logical chain yet. When molecules pursuing orbital courses about the earth are forced across the limit of control into orbital courses about the sun, they must return to the points of their last collisions, in the natural course of things, if they do not suffer interferences or diversions of one kind or another in the meantime. These points of last collision were within the sphere of the earth's control. So these molecules, in their new solar orbits, will come back into or cut across the earth's sphere of control. There are thus definite possibilities that some of these returning molecules of the earth's atmosphere will encounter molecules of the earth's atmosphere and that the collision will be such as to force them again into orbits under the control of the earth or into courses that will plunge them into the denser atmosphere about the earth. Here is a definite method of recovery of escaped molecules after they have become members of the sun's ultra-atmosphere.

In like manner, molecules that revolve about the sun as members of its ultra-atmosphere, even though they were never previously members of the earth's atmosphere, may cut through the earth's sphere of control and thus be liable to a collision with an atmospheric molecule and as a result be incorporated in the earth's atmosphere. The same logic and the same laws that we have found applicable to the atmosphere of the earth are applicable also to the atmosphere of the sun. It must have its krenal ultra-atmosphere and its orbital ultra-atmosphere. These ultra-atmospheres of the sun are required by the logic of the case to reach out not only to the earth, but to the limits of the sun's sphere of control. This lies very far outside the outermost planet. There is thus not only a threefold phase of the solar atmosphere, as well as of the planetary atmospheres, but *the krenal and the orbital phases of the solar atmosphere envelop the atmospheres of all the planets.*

It thus appears that passages of molecules from the sphere of control of the sun into the sphere of control of the earth, and the reverse, are inevitable consequences of the kinetic theory of gases, unless there be agencies that contravene to prevent the logical consequences of kinetic laws.

It is important to observe further that such interchanges of molecules tend to establish an equilibrium between the ultra-atmospheres of the earth and the ultra-atmospheres of the sun; for if one of these atmospheres becomes more plethoric than is concordant with its relations to the other, it will inevitably feed more molecules into the leaner atmosphere than the latter will return to it. In this, then, there is a reciprocal process that tends to equate the loss and gain between the planetary ultra-atmospheres and the solar ultra-atmospheres. This reciprocity tends toward the maintenance of a stable condition in both.

SUPPLEMENTARY AGENCIES

We have now followed far enough for our purposes the intricate system of actions and reactions that take place in the atmospheres of the solar system. We have considered them, however, wholly apart from incidental or co-operative agencies, though we have referred to the possibility of contravening agencies.

There are probably no strictly contravening agencies, but among the other agencies that act on the atmosphere there are some that interfere with or modify the systematic development of the ultra-atmospheres we have just considered. One of these, to which passing attention must be given, is the pressure of light. As in the case of most newly discovered agencies, there is a current tendency to call on light-pressure for services it is incompetent to perform. It is, however, probably a factor in the feeding and depletion of planetary atmospheres. It is probably not a very competent factor, since the diameters of molecules are too small to permit the wave-action of light to act upon them efficiently except perhaps in case of selective absorption. In so far as light is absorbed by the molecules, the propulsive energy of the light is felt, and to that extent the molecules are driven in the direction of light-propagation.

The diminution of light with distance follows the same law as the distribution of gravitation. There are various ways in which light is lost as it progresses, while gravity is not known to suffer loss. It follows that the power of light-pressure compared with gravity is greatest near the luminous body from which it originates. Its dispersive power must be greatest there, and so the very fact that the sun and the stars hold their gases and apparently have held them for eons in spite of the light-pressure seems to imply that the latter can be at best only a limited agency of dispersion. There seems no ground, therefore, to treat it as more than a co-operating agency in atmospheric feeding and dispersion.

So far as it is potent at all, its first function should be the dispersion of the outer atmosphere of the sun. It should aid in counteracting the gravity of the sun and hence in facilitating the development of vaulting and orbital paths, and the enlargement of the orbital paths as a part of its systematic work in causing molecules to move out from the sun. In the course of these outward movements the earth should be encountered and some of the solar molecules should be captured by it. Such action would constitute a systematic feeding of the earth's atmosphere at the expense of the sun's atmosphere.

On the other hand, the light of the sun traversing the sphere of the earth's control transversely should tend to drive away some of the molecules of the ultra-atmospheres of the earth, and thus have a depleting effect on the earth's atmosphere. We may assume for present purposes that the feeding and depleting effects offset one another more or less completely, though, theoretically, the probabilities of gain seem to lie somewhat with the earth, a dark body, as against the sun, a brilliantly luminous body.

It is highly probable that electric action plays an important part in the distribution of molecules in the rare outer zones of the atmospheres of the sun and its planets. Rather definite hints of such effects may be gathered from auroral phenomena, electric storms, the behavior of the trains of meteorites between forty-five and sixty miles above the surface, the ionizing effects of ultra-violet light, and other phenomena, but data for confident treatment are as yet wanting.

There is, however, a single suggestion of such vital relations to the organic function subserved by the atmosphere that a very tentative statement of its nature may be justified in this connection.

There is reason to believe that the violent explosions of the sun that give rise to "eruptive prominences" often force molecules far out into interplanetary space and that the electric fields of the sun, of the prominences in particular, and of the earth have a selective effect on charged molecules. According to the recent brilliant determinations of Hale and his colleagues, the surface charge of the sun is dominantly negative, while in connection with the sun-spots, to which the prominences are related, there are great vortices of gases bearing high negative charges. On the other hand, according to recent determinations, the atmosphere of the earth is dominantly positive, while the earth-body is dominantly negative. These suggest that electric differentiations obtain in the solar system with perhaps alternate arrangements of the electric charges. However this may be, these charged tracts of the sun and of the earth form a suggestive working combination. The sun's negative charge must tend to draw to itself molecules carrying positive charges and tend to repel molecules carrying negative charges. The positive charge of the earth's atmosphere, on the contrary, tends to draw to it molecules of negative charge and repel those of positive charge. It is well known that under the ordinary conditions of electrolysis a composite molecule in suffering dissociation yields a positive charge to one class of elements and negative charge to another class and that the atmospheric constituents belong with the latter rather than the former, oxygen in particular taking the negative charge. The experimental evidence, unfortunately, is very limited in cases where the dissociation takes place in other than aqueous solutions, particularly in cases analogous to that in hand. However, J. J. Thomson found in certain experiments that oxygen took up free electrons while in the course of projection. There is also some additional support for the assumption that, by means of the electric dissociation which doubtless attends the solar eruptions, oxygen and perhaps other atmospheric constituents receive negative charges. If so, they suffer electric, as well as mechanical, propulsion from the sun. Such elements as may receive positive charges are drawn back to the sun. The constitution of the atmosphere of the sun and stars seems to be fairly in harmony with the suggestion of a positive electric segregation, while the nature of the atmosphere of the earth suggests a negative segregation.

There are several other sources from which high molecular velocities might arise in the outer atmosphere, as, for example, the plunge of meteorites and the carrying aloft of minute radioactive particles, but their quantitative value is too questionable to require consideration here.

The value of some of these undetermined factors in promoting the escape or the accession of molecules is only conjectural at present, but it seems unsafe to ignore them. No computation relative to the rate of escape of the earth's atmosphere, however accurate in its mathematical processes, has any claim to serious weight if it does not recognize all the factors. It seems especially untrustworthy if it does not give full consideration to the ultra-atmospheres of the earth and of the sun, and to their interplay. To include all these certainly renders the case intricate in the extreme. The uncertainty as to the values of the several co-operative factors lends especial embarrassment to all efforts in computative lines. The geo-naturalist endeavors to circumvent the intricacies of these complications by search for vestiges in which nature herself, having actually made the correlation, records her composite equated result.

Respecting the power to acquire and retain atmospheres, the bodies that attend the sun give a fairly clear answer, as Stoney has urged; the great planets have great atmospheres, the terrestrial planets have small atmospheres, the planetoids and satellites little or none at all. Jupiter has undoubtedly many times more atmosphere than all the terrestrial planets, planetoids and satellites combined. Among the terrestrial planets the atmospheres seem to be graded strictly in the order of masses, and somewhat nearly in proportion to mass when modified for distance and temperature. The law of Stoney that atmospheres are apportioned to masses has strong naturalistic support. Measurably so has the philosophy of interchange to which we assign the maintenance of such atmospheres. So far as the special features of maintenance are concerned, the naturalistic record of composite results appears in a long line of phenomena, partially astronomical, partly biological, more largely geological, to which some allusion has been made in the Introduction. The analysis and interpretation of these is scarcely less embarrassed by intricacies and unknown factors than the computative method. Many aspects of the problem of atmospheric maintenance must, for the time being, remain open. The deployment of the problem, however, may serve to restrain unwarranted claims to conclusive determinations.

THE ATMOSPHERIC TEST OF THE LAPLACIAN HYPOTHESIS

The revisions and extensions of the kinetic view of our atmosphere involved in the foregoing sketch, particularly the orbital atmosphere, the progressive mode of escape of molecules, the interchange between the solar and the terrestrial atmospheres and the equilibrium between these, followed rather than preceded the atmospheric test of the Laplacian hypothesis to which we are now prepared to turn. The test started with the older kinetic views of the atmosphere, particularly that phase of them advanced by Dr. G. Johnstone Stoney.[3] The very suggestion of the test sprang from Stoney's doctrine that a planet could not hold an indefinite amount of atmosphere but only an amount proportionate to its mass when other conditions are duly considered. The considerations we have added seem to supplement and strengthen this point of view, and they are here given in anticipation of the atmospheric test of the validity of the gaseo-molten theory of the origin of the earth.

To make the test critically it was thought necessary to take into account the fact that the parabolic velocity of the earth—which, following current practice, I then took as the critical velocity of molecular escape—decreases with the altitude, and that it varies with the rate of the earth's rotation.[4] To assist in weighing these factors, Dr. F. R. Moulton was kind enough to prepare for me tables of the variations of the parabolic velocity for various altitudes with adaptations to a stationary earth, to an earth with the present rotation, and to an earth rotating in one hour and twenty-four minutes, this last being the rate at which the rotation would cause separation by centrifugal action at the equatorial surface. Such a rotation would throw the atmosphere off and initiate disruption of the earth, so that there was no need to consider any higher rate of rotation. Dr. Moulton added formulas by which the tables could be extended or modified to suit such other cases as might be selected.[5] Dr. A. W. Whitney prepared for me a table of the velocities that would be acquired by the several constituents of the atmosphere for a series of temperatures, and also a table of the frequency at which a certain percentage of molecules would acquire specified velocities.[6] With these aids a series of representative cases was tested.

One of the gravest difficulties encountered in trying to make a test that might be taken as conclusive lay in the uncertainty as to the precise state of things in the upper atmospheric levels, an uncertainty which was then particularly great. However, it is sometimes possible to deal with a question in a really conclusive way even when the precise conditions are indeterminable, if it happens to be possible to bring under test a graduated series of representative cases whose combined range is wide enough to cover all supposable cases. If the tests show that none of these is tenable, the entire range of cases is effectively excluded, and it is immaterial what the details of the actual case may be.

In the case in hand, the tests did not make it altogether certain that a white-hot earth could not hold the larger part of the great atmosphere assigned it, for the relatively brief period during which the putative white-hot stage would last, for time is a vital factor in the loss of an atmosphere. It did appear, however, quite doubtful whether so much water-vapor as was postulated could have been held, even through the white-hot stage, because its molecular activity would have been exceptionally high, as the molecule of water gas is relatively light. There would also have been liability to dissociation, which would give rise to still higher molecular activity.

TEST CARRIED BACK TO POSTULATED GASEOUS GLOBE

As the tests applied to the white-hot stage thus seemed to fall somewhat short of complete conclusiveness, the method was applied to the next earlier stage of the earth postulated by the Laplacian hypothesis, when the earth was entirely gaseous and when the molecular activities and the temperatures were still higher and the chemical dissociations probably more extensive. Not only were the temperatures at that stage high enough, by hypothesis, to keep even the refractory substances of the earth in a vaporous state, but the vast gaseous globe in its gaseous state must have been much larger than in its later stages, and hence the velocity required for the escape of molecules from its outer border was proportionately less. There seemed good reasons for thinking that hydrogen would be liberated by dissociation at such temperatures. Now the earth is scarcely competent to hold hydrogen permanently at present temperatures and at the present atmospheric surface where the gravity effect is greater than it could have been at the surface of the postulated gaseous globe. These and other considerations seemed to make it extremely doubtful whether water-vapor, or its dissociated constituents, could have been held under control on the outer border of the extremely hot gaseous spheroid. This conclusion is strengthened by the probability that such a very hot gaseous spheroid would be affected by explosive eruptions, somewhat as the sun is. Violent ejections of such a type would without doubt have greatly aided the discharge of the more active gases.

TEST APPLIED TO THE POSTULATED NEBULAR RING

But, to cover the last remnant of doubt, the test was carried still another step backward and applied to the postulated stage at which the substance of the earth and moon formed a gaseous ring, recently parted from the rotating nebula. The assigned diameter of the ring was of the order of the orbit of the earth. It may help to an appreciation of the gravitative feebleness of this ring, to note that even if it were reduced to a solid state of the mean density of the present earth, it would have a cross-section of only about 40 kilometers. It is further to be noted that the center of gravity of the ring would be at the center of the sun and the self-gravity of any section of the ring would be exceedingly feeble. When all that these two factors imply was duly considered, the case became eminently decisive. A ring of gas, such as the Laplacian hypothesis postulates as the parent of the earth, with a temperature high enough to keep the refractory substances that make up most of the earth in the form of a gas, could not have held itself together by its own gravity in the collisional relations of a gas, most certainly not so far as the atmospheric constituents were concerned.

When, therefore, the results of this ultimate test had been duly pondered, one of two alternatives seemed imperative: either to conclude that the kinetic theory of gases was seriously wrong, or that such a ring of gas as the Laplacian hypothesis postulated could not have held in gaseous relations the waters of the oceans or the constituents of the air, nor perhaps even the rock substances of the earth.

At the time the test was made there was ground for some reserve relative to the soundness of the kinetic theory of gases. It seemed wise, therefore, before setting aside the Laplacian hypothesis, to seek lines of test that did not rest on the trustworthiness of the kinetic theory of gases. These will claim attention later. Before passing on, however, it is proper to remark that there now remains no reasonable ground for doubting the essential verity of the kinetic theory of gases. The test based on this theory was, therefore, really much more conclusive than it was held to be at the time it was made.

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CHAPTER II

VESTIGES OF COSMOGONIC STATES AND THEIR SIGNIFICANCE

There was a time when that part of the history of the earth which antedates human records was held to be purely speculative. It was easy to say that no man had then lived to witness terrestrial events and they could not be humanly known. Pre-human history was, indeed, merely speculative until a certain stage in the growth of insight had been reached. Man had lacked the acumen to read the record written in the earth itself by the processes of its own formation. Ignoring this automatic record, he indulged in speculative fancies in lieu of serious inquiry. But he has since learned that the automatic vestiges of creation may be read with great trustworthiness so far as their larger import is concerned; he is learning almost daily of successful advances in deciphering the less readable phases of the record. It is not unnatural that the autobiography of the earth should vary greatly in the clearness and cogency of its revelations. The most tangible part of the record is found in material vestiges, in footprints of processes and footprints of living creatures, in ripple marks, in mud-cracks, in fossils, in the necks of decapitated volcanoes, in the roots of vanished mountains, and in the multitude of distinctive marks left by former activities. The laying down of the strata one upon another in orderly sequence—the rings of terrestrial growth—and the great lines of the earth's architecture are replete with historic testimony of unimpeachable fidelity. But these materialistic records are by no means the only ones, nor always the most important ones; there are *dynamic* relics that are as truly vestiges of processes once in progress as are fossils or strata. These may be rotations, revolutions, inclinations of axis, ellipticities, or any other of the activities, attitudes, or configurations that make up the subject-matter of celestial mechanics and of terrestrial dynamics. These residual activities, attitudes, and configurations may imply past events as specific in their natures and as illuminating in their historic significance as those more familiar material vestiges of earth-processes by which its history is now so confidently read. The testimony of these dynamic vestiges may often seem to be more difficult to interpret than the meaning of the material vestiges. To many, perhaps, they seem more elusive in their nature and less cogent in their significance, but, in some instances, and in some respects at least, the opposite is really the truth. In not a few cases the dynamic testimony is singularly convincing. Dynamic evidence has indeed its limitations, as have all kinds of testimony, but, in its own field, it is often surpassingly clear and altogether decisive.

As the history of the earth is traced back to those early stages in which its substance took on physical states other than those it now bears, the special forms that give meaning to the material record vanish, and little recourse is left but to turn to the dynamic record which, fortunately, is often less mutable, under the vicissitudes that affect earth-substance. The method of inquiry, however, remains substantially the same. The progress of inquiry from sole reliance on human records to the interpretation of the testimony of the rocks was not a lapse from a realm of determinate certainty to a realm of equivocal speculation, as some laymen were wont to say when earth-science was in its infancy; nor is the passage of inquiry from the use of material records to the study of the dynamic record a plunge from firm science into nebulous speculation, as some devotees of science permit themselves to hint even now; it is merely an onward step from the reading of the more tangible and the easier to the reading of the less tangible and the more difficult. No doubt the reading of the easier and the more familiar is to be received with greater confidence, for the time being, than the reading of the less tangible and the more difficult. The need for severe scrutiny before acceptance is without doubt more imperative in dealing with the latter than with the former, but close scrutiny is needed in both cases; it is needed always and everywhere. The reading of the later history of the earth and the reading of its earliest history are parts of the same endeavor and proceed by similar processes. The indispensable factor in both cases is a scrupulous search for vestiges of what has taken place and a studious endeavor to find the interpretation thereof.

THE VESTIGES IN THE SUN

When, as stated near the close of the last chapter, the atmospheric test of the Laplacian view of the origin of the earth seemed to give results seriously adverse to that hypothesis and to make it wise to test, in turn, the trustworthiness of this test itself by finding some other mode of scrutiny free from dependence on the theory of gases, attention happened to be first drawn to the slowness of the sun's present rotation. This rotation is an inheritance from the rotation the sun had when the planets were formed, and is as truly a vestige of creation as the Silurian formations, the Cambrian trilobites, or the Paleozoic Alps. Attention was later directed to the obliquity of the sun's axis, another dynamic vestige of like fundamental nature.

It is one of the basal postulates of the Laplacian hypothesis—and, substantially, of all other hypotheses that belong to the same genus—that the nebula which evolved into the solar family had a certain rotation when it was in its most expanded condition, and that, as it cooled and shrank, its rate of rotation increased in accordance with mechanical law to preserve the value of its rotatory momentum, technically its moment of momentum. The constancy of the moment of momentum in such a rotatory system is one of the best established principles of mechanics. It is inevitably involved in all centrifugal theories of celestial genesis, and must indeed enter radically and consistently into any cosmogonic theory, whatever its nature, if such theory is to have any claim to serious consideration. As such indispensable principle, it is susceptible of being made a criterion of the highest value in testing the validity of cosmogonic tenets. It is peculiarly applicable to the tenets of hypotheses confessedly founded on it. In the application of this principle, it was held under the Laplacian hypothesis—by implication, if not by open declaration—that when the parent nebula by cooling had shrunk to a diameter of about five and one-half billion miles—the velocity at its equator being then three and four-tenths miles per second—a ring was separated by centrifugal action which afterward gathered into the planet Neptune. When, two stages later, the nebula had shrunk to a diameter somewhat less than a billion miles—the velocity then acquired being about eight miles per second at the equator—a more massive ring was supposed to have been separated which later formed the great planet Jupiter. When, again omitting two stages, the nebula had shrunk to a diameter about equal to that of the orbit of the earth—its equatorial velocity having risen to eighteen and a half miles per second—another ring was separated which formed the earth. When the nebula had shrunk to a size comparable to the orbit of Mercury, its equatorial speed had risen to about twenty-nine miles per second. As shrinkage continued after the separation of Mercury, the principle requires that proportionate increases of rotatory velocity should have ensued. Further separations of equatorial matter and further formation of planets might, in complete consistency with the hypothesis, be presumed to have taken place and, indeed, were presumed to have taken place by the astronomers of the last century. They made diligent search for inner planets at times of solar eclipse when the glare of the sun, that might ordinarily obscure small planets in its vicinity, was cut off. An eminent astronomer even announced the discovery of such a planet and named it Vulcan, but the observation proved illusory. Now, if Vulcan had proved to be a reality, and if the radius of its orbit had been a million miles, its velocity in its orbit, if circular, should have been about 170 miles per second, and the equatorial velocity of rotation of the parent nebula at the time of the planet's supposed separation should have been the same. The further contraction of the nebula to the present radius of the sun should have given it an equatorial velocity of 270 miles (435 kilometers) per second. *But the actual velocity of the sun's equator is only about one and a third miles (two kilometers) per second.* In other words, the actual velocity is only about one-half of 1 per cent of the theoretical requirement. Here then is an enormous discrepancy. The discrepancy is the more notable in that it arises from the very principle on which the hypothesis is founded. Interpreted as a dynamic relic of the sun's past history and as an index of its genesis, this inconsistently slow rotation seems to imply that the sun is not the residual product of any such a system of progressive separations as the Laplacian and similar centrifugal hypotheses postulate. It is not easy to see how this conclusion can be escaped, unless it can be shown that some competent agency, acting as a break, came into action after the separation of Mercury and was efficient enough to reduce the rotation of the sun to a two-hundredth part of the velocity toward which it had been trending up to this stage in accordance with one of the best established laws of mechanics. There is an inherent difficulty in seeing just how any such agency, or the source of any such agency, could have existed in the system and have remained in abeyance during the whole active period of planetary evolution so as to permit rotation to increase systematically until all the planets were cast off, and then, but then only, have come into action of an opposite order and of so high efficiency that further contraction should not only have failed to sustain the previous habit of steadily increasing rotation but should have reversed the effect with so much potency as to bring the rotation down to a very small fraction of what had already been attained.

However, a possible agency working somewhat in this strange way must be considered. So long as the planetary matter remained in the form of rings, its attraction had no deterrent action on the sun's rotation, but as soon as the rings were gathered into concentrated masses, as the hypothesis assumes they did, each of these masses tended to develop tides in the sun, and these tides acted as brakes on its rotation. Now, in the first place, it must be noted that as soon as any planetary mass began to act in this way, it tended to stop the planet-forming process. Too much efficiency of this kind on the part of the outer planets would have forestalled the formation of the inner planets. Now the time at which such masses could be most effective was in their earliest stages, for then the solar body was largest and its proximate side was nearest to them while its distal side was farthest from them. Furthermore, the tides of these first-formed masses were then least neutralized by the tides of the later-formed masses. The planets are distributed about the sun by their different rates of revolution and are rarely, if ever, all on a single side of the sun, or on the opposite sides, so as to conjoin their tidal effects. Their distribution is constantly changing, so that any distribution that tends to tidal efficiency is merely temporary. As a result, their several tides neutralize one another in large degree and the residual effect is small. The subject of tidal influence in the evolution of our planetary system has been elaborately investigated by Sir George Darwin, and as his working hypotheses were such as to give to tidal effects their maximum probable values, his results are assumed to be conclusive. He found that the utmost assignable effects of all the planetary tides upon the rotation of the sun, and upon the reciprocal retreat of the planets, is so trivial as to be quite negligible. Even if the age of the system be greatly extended beyond current estimates, the evolutionary value of the planetary tides does not rise to appreciable moment.[1]

As the sun is constantly radiating away enormous quantities of heat, it is improbable that the retarding action of the planetary tides has been at any time equal to the accelerating effect of the sun's contraction, even if the solar contraction is much slower than was formerly supposed, owing to sources of heat then undiscovered. It is, however, impracticable to determine this positively in the present state of knowledge relative to the sun's sources of heat.

If there were any doubt as to the incompetency of the planetary tides to account for the great discrepancy between the actual rotation of the sun and the theoretical rotation it should have under the Laplacian hypothesis, this doubt should prompt us to a search for other grounds on which to test the hypothesis by the application of fundamental principles, for discrepancies are almost sure to insinuate themselves under the mantle of any hypothesis that does not tally with the historic reality. While no doubt is here entertained as to the incompetency of tidal action to explain the great discrepancy disclosed by the rotation of the sun, an independent line of inquiry, undertaken to cover, so far as possible, any weaknesses that might be supposed to lurk in this argument and in the preceding atmospheric test, will be the subject of the next chapter.[2]

It was only after such a supplementary inquiry had been made that the implications of the second of the sun's significant vestiges, the inclination of its axis to the planes of the planets, arrested serious attention, and so, if strict historic sequence were followed, this feature should be discussed later; but while we are considering the dynamic vestiges of the sun, it is convenient to refer briefly to the inclination of its axis. It must be quite obvious to everyone familiar with ordinary mechanics that, if the sun, by reason of its rotation, "threw off" parts of itself by centrifugal action, they should have taken paths lying in the plane of its equator; much more is this evident if the center of the nebula simply shrank away from the outer rim, the true picture. This holds whether these parts were left behind as rings or as individual particles, or in any other natural manner.

But as a matter of fact the plane of the earth's orbit is inclined 7° 15′ to the plane of the sun's equator. The orbits of all other planets are also inclined to the plane of the sun's equator, some more, some less than this; some of the planetoids very much more than this. If, however, these varying inclinations were such as completely to offset one another so that the mean plane coincided with the sun's equator, the conditions of the theory might still be regarded as fulfilled; but the "invariable plane" of the planetary system which summarizes the total inclination values of all the planetary orbits is also inclined to the sun's equator 5° ±. While this is not a very large angle, the inertia represented by the motion of the planets is so enormous that even this small deviation represents a rather grave discrepancy between theory and fact, though it does not rise to the serious nature of the preceding discrepancy. It will be necessary to recur to this feature when later we turn from destructive criticism to the much more difficult task of constructing a hypothesis to meet, if possible, this and the many other significant features of the actual system.

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CHAPTER III

THE DECISIVE TESTIMONY OF CERTAIN VESTIGES OF THE SOLAR SYSTEM

When, as recited in the first chapter, the gaseous factor of the nebular hypothesis seemed to betray serious weaknesses under the tests of the kinetic theory of gases and when, as recited in the last chapter, the centrifugal factor seemed to disclose even more serious incongruities when tested by the hypothesis' own fundamental tenet, there arose a pressing need to seek other and, if possible, more rigorous tests. It could not be lightly assumed that a hypothesis which had been so widely accepted for a century was thus fatally weak in its own fundamentals. It was more natural to assume that the inquirer had himself fallen into error or misconception. However, the call to proceed till the error or the misconception, wherever it lay, should be disclosed was none the less imperative. The call was perhaps all the more urgent because the weaknesses of this, the leading gaseous hypothesis, seemed to involve all other gaseous hypotheses as well as all quasi-gaseous hypotheses and perhaps all centrifugal hypotheses. Indeed, it seemed to raise doubt as to the possibility of even framing any centrifugal hypothesis that could fit the facts of our planetary system. This does not of course imply that some other planetary system might not arise from centrifugal separation, but merely that *our* planetary system, being what it is, did not arise in that way.

Dr. Moulton had, as previously stated, furnished me with tables and formulas of parabolic velocities for the earth, under different conditions of volume and rotation, as an aid in the gaseous inquiry, and I had sought his good opinion relative to the significance of the sun's slow rotation. He was now good enough to join seriously in the inquiry and to take the leadership in testing the tenets of the Laplacian hypothesis by means of the laws of dynamics, a line of investigation in which the skill of a master in celestial mechanics carried a value of the highest order. The new tests were singularly fertile in disclosing discrepancies.[1]

SPECIFIC DEFECTS OF THE LAPLACIAN HYPOTHESIS

1. For a first trial, the nebula postulated by the Laplacian hypothesis as the parent of the solar system was restored by Dr. Moulton as faithfully as possible by a theoretical conversion of the entire mass of the present system into gas and by assigning to it such a deployment as would be required by the accepted laws of gaseous distribution. In doing this he endeavored to give the hypothesis the benefit of every doubt and to allow a liberal margin of safety in every case of quantitative uncertainty. To this restored nebula he assigned the full value of all the momentum the system now possesses. Comparison was then made between this representative nebula and the actual solar system in regard to the respective values of their momenta. These values are fundamental in nature and should tally closely with one another if the Laplacian hypothesis were true.

The first stage selected for comparison was naturally that at which the restored nebula had, by hypothesis, shrunk to the size of the orbit of Neptune and was ready to cast off a ring to form that planet. The value of the momentum in the nebula and the value of the momentum that would be necessary to bring about the separation of the postulated ring by centrifugal action were each computed and were found to be widely discrepant, the momentum of the nebula having less than a two-hundredth of the value required for separation. A similar trial was made at the stage when the matter for Jupiter should have parted from the nebula, and it was found that the nebula then had less than a hundred and fortieth of the momentum required to separate a ring. At the stage assigned for the setting off of the earth-moon ring, the nebula had about an eighteen-hundredth of the momentum necessary. At the Mercury stage, it had about a twelve-hundredth. It will be seen that these are discrepancies of a very high order and are quite comparable in this respect to the discrepancy disclosed by the sun's slow rotation.

While these inquiries of Dr. Moulton were entirely independent of previous investigations in like lines, as was natural from the special way in which he was led to make them, it was found later that Babinet had detected discrepancies of the same type many years previously.[2] Though his conclusions had been reached in a somewhat analogous way, the methods pursued were not identical. It does not appear from what can now be learned that Babinet pushed his inquiry so far as to become convinced that the discrepancies were fatal to the Laplacian hypothesis. On the contrary, he seems to have regarded the incongruities merely as difficulties which must be met in some way by the hypothesis which he appears to have continued to accept.

2. In the tests of Dr. Moulton, each stage in the evolution of the nebula was considered by itself, such masses as had been separated previously to form planets outside the one under consideration being subtracted from the nebula, following in this, as in other respects, precisely the terms of the Laplacian hypothesis. Each case was thus, in some sense, an independent one. To apply the test in a somewhat different way, it was assumed that the whole mass of the system remained in the nebula until the rate of its rotation became sufficient to force the separation of the rim as a ring in accordance with the assumptions of the hypothesis. It was found that the centrifugal component would not rise to equality with the centripetal force of gravity until after the nebula had shrunk within the orbit of the innermost planet.

As all these tests were based on well-established dynamical laws, the conclusions could not fairly be regarded as much less than rigorous, except perhaps in so far as they were dependent on the accuracy of the restoration of the nebula, which was guided by the accepted law of distribution of gases. This law, while probably rigorous under ideal conditions, shows some tendency to break down in cases where the state of the gas is near the border line that marks the transition from the gaseous state to some other state; but in all known cases, the departures from the strict terms of the law were found to be such as to bear *against* the hypothesis, so that here, as in other cases, the assumption of the complete integrity of the law gave the hypothesis the benefit of the doubt. The critical reader will readily see that the true law of distribution might vary widely from the accepted law without removing in any large measure the great discrepancies disclosed.

3. Although there was thus no tangible ground for apprehending that any falling away from the law of distribution of gases could essentially weaken the rigor of the conclusions reached by Moulton's dynamical inspection, it seemed none the less desirable to find a test whose working factors were not derived from the law of distribution of gases, and thus cover by such alternative inquiry any doubt that might seem to arise from this source. I endeavored to find such a test in a comparison of masses and momenta as they now exist.[3] The method proceeded on the assumption that the masses and the momenta alike remained essentially constant throughout the evolution, an assumption inherent in the principles of the Laplacian hypothesis. Exchange of momentum between the members of the system is not, however, excluded, and there will be something to say of the possible extent of this after the mode of trial and its results have been outlined. The method may be concretely illustrated in the case of the great planet Jupiter which fairly represents the general tenor of the results in other cases. Jupiter, including his satellites, now carries a little less than one-thousandth (1/1,024) of the mass of the solar system exclusive of the planets outside Jupiter which do not enter into this comparison. The mass of the nebula just before the Jovian ring was separated from it, was, according to the Laplacian hypothesis, identical with the combined masses of Jupiter and the bodies within

its orbit. For the purposes of the inspection the momentum values now carried by the Jovian family and the bodies within were taken from the computations of Sir George Darwin and thus the results were made to rest on authoritative data wholly independent of the computations of my colleague. Now the reader will find little difficulty in forming a mental picture that roughly represents the proportions of momentum and of mass in the different parts of a symmetrical rotating body such as the nebula must have been; at least he can picture disproportions in the different parts so great that they would not be developed in a natural evolution, and so he may limit to his own satisfaction the *range* within which the true case must lie, without assuming to know precisely what the exact fact is. If skilled in mechanics, he may use a series of hypotheses that fix more definitely the range within which the true case must fall. The nebula at the initial stage of partition must have formed an oblate spheroid rotating at such a rate that the outer one-thousandth part was just ready to separate to form Jupiter and his moons. The next thousandth part lay just inside that and was rotating at a proportional rate, which was somewhat slower; the next thousandth lay next within and was still slower, and so on down to the last thousandth which had almost no rotatory momentum at all. The value of the momentum in each case is measured by the product of the mass, the speed, and the length of the arm on which each part was rotating, and the comparison is to be between the outer thousandth and the *sum* of all the remaining nine hundred and ninety-nine thousandths. If the reader has fixed upon the highest proportion of the total momentum that could possibly, in his judgment, be carried by the outer one-thousandth part of the nebula, he will be prepared to appreciate how far the hypothesis is credible when, by recourse to the data of Sir George Darwin, it is found that Jupiter and his moons now carry 96 per cent of the whole momentum, leaving to the remaining nine hundred and ninety-nine parts only 4 per cent. In some respects this remarkable disproportion is quite as convincing evidence that Jupiter was not separated by simple centrifugal action as the more rigorous determination by the previous method which showed that the existing momentum is one hundred and forty times more than the same matter would have carried in the restored nebula.

When this alternative method was applied to other planets, similar disproportions between masses and momenta were disclosed; in some cases even a greater relative disproportion was revealed than in the case of Jupiter.

4. Inspections of the foregoing kinds that direct specific attention to the conditions under which separation should take place force the conviction that a certain regularity and symmetry in respect to the masses of the successive rings must have resulted from the centrifugal process, if it obtained. But very striking irregularities in the masses of the planets are observed. If the mass of the earth be taken as unity, the order of the planetary masses from the outermost to the innermost is 17; 14.6; 94.8; 317.7; 0.1073; 1; 0.82; 0.0476. This irregularity has of course long been known and the incongruity recognized but not thought fatal.

5. Rings shed from the rim of a rotating spheroid should have been strictly circular when they were first formed, and no wide departures from circularity should probably have followed in the course of subsequent evolution. The orbits of most of the planets approach fairly closely to circularity and no severe indictment of centrifugal hypotheses can be based on the ellipticities observed, though some of them are rather notable. The orbits of the planetoids, however, are often much more eccentric, and their planes diverge more notably from the invariable plane of the system. The attraction of their powerful neighbor Jupiter is sometimes held responsible for this. There is, however, a singular fact about the orbits of the planetoids that is not met by this plausible hypothesis. Bodies shed from a nebula by centrifugal action should have orbits strictly concentric with one another; no orbit should loop through any other. The orbits of the planetoids, however, are so singularly interlooped that, if they were solid rods, the lifting of one would lift the whole group.

6. If we turn to the supposed evolution of the satellites from the planets by centrifugal action, some features as strikingly incongruous as any of the preceding are encountered. Under the centrifugal theory, all the satellite rings should have rotated precisely as their parent nebulae did, and when the rings were condensed into satellites these should have revolved in the same direction as their primaries. Each inner ring should have rotated in less time than the rings outside it, while the central body should have rotated in a shorter period than any ring. The principle is the same as that already considered in relation to the rotation of the sun. But

Phobos, the inner satellite of Mars, revolves around that planet more than three times while the planet rotates once. This is a very singular, telltale vestige of Mars's early history. While this anomaly has been known ever since Hall discovered the satellites in 1868, and has been recognized as puzzling, its force was largely avoided or palliated by the hypothesis that the rotation of Mars was indeed high at the outset but has been so reduced in the course of time by the tidal action of its moons that the present strange state of affairs was reached. Nolan, however, insisted that this explanation was inadequate.[4] Moulton added piquancy to the anomaly by pointing out that the little bodies which make up the inner border of Saturn's innermost ring revolve in a period only about half that of Saturn's rotation. Moulton further pointed out that, even if a tidal scheme could be made to fit the case of Mars, it would not, at the same time, fit the case of Saturn, unless it were assumed that Saturn is something like three thousand times as old as Mars.

7. Though it is not in proper historical order here, this is a convenient place to remark that three even more telltale cases of strange behavior on the part of satellites have been discovered since we were led by the foregoing and other considerations to abandon the centrifugal theory of satellite origin and to adopt a new one. Among the new satellites that have been discovered by photography, it appears that Saturn has one and Jupiter has two that revolve in a *retrograde* direction *contrary to the rest.* Nothing would seem more obvious than that a planetary spheroid, rotating so fast as to shed a series of rings by centrifugal action to form

satellites, should impart to them all its own direction of motion. Such a result is so obvious that it was formerly taught that a single exception would be absolutely fatal to the Laplacian theory, and the writer was so instructed in his college days. It now appears that, while the majority of the satellites revolve in the same direction as their primaries, a minority take the opposite course, and that these contrary habits are found in the same family of moons in two cases.

8. Among the older objections to the ring theory was the inference that a gaseous spheroid would not cast off a definite ring, even if its rotation were so increased that separation in some form was inevitable. It was felt that the molecules would go off separately, or at the most in small groups, and that such small separations would follow at short intervals, so that the whole would form a disk rather than a series of distinct rings. The molecules of gases are held together by gravity in spite of a tendency to fly apart by reason of rebounds from collisions with other molecules, and hence so soon as gravity at the outermost rim of the rotating nebula was neutralized by the increasing centrifugal force, the molecules should have gone off individually into orbits. There was no agency to hold them back until the other molecules requisite to make up a ring great enough to form a planet should also have reached the state requiring separation. A ring of sufficient magnitude to form the greater planets should have had some millions of miles of depth and the differences in the ratio of rotation to gravity in its outer and in its inner edges respectively should have been rather large. This objection is so obvious that some surprise may naturally

be entertained that the formation of rings was ever made a part of the hypothesis. There seem to have been two reasons, doubtless seemingly cogent at the time, for the introduction of the ring feature.

One of these was a supposed logical necessity to meet the facts of planetary rotation. All the rotations of the secondary bodies of the solar system were in the same direction as their primaries, that is *forward*, so far as known when the Laplacian hypothesis was framed. It was reasoned that if a ring rotated *as a unit*, the outer part of which moves faster than the inner, the rotation of the globe into which the ring gathered would also be forward; but if, on the other hand, the ring were made up of small bodies revolving independently, the inner bodies in this case moving faster than the outer, as they must, the rotation of the resulting globe would be in the opposite or *retrograde* direction. This cogent logic seemed to warn everyone away from any theory that started with particles pursuing independent revolutions. To all such hypotheses it seems to have served as a lion in the way, effectually warning off cosmogonic pilgrims. The warning seems to have been religiously heeded throughout the last century. The question will arise later whether it was anything more than the skin of a lion, but let that pass here.

The other reason was naturalistic. The rings of Saturn were very naturally thought to be vestiges of the evolutionary process, and, correctly interpreted, they were certainly entitled to be so regarded. There can be little doubt that they were really the foster-parents of the ring theory. In Laplace's time, it was not unnatural to suppose that they were gaseous. It required the acumen of later mathematics and the analyzing power of the spectroscope to prove that the rings are in reality composed of little bodies revolving independently, satellitesimals, if you please, the very class of bodies that were supposed to give rise to *retrograde* rotations. For a hypothesis built up so naturally in response to the apparent teachings of such lovely celestial objects as the Saturnian rings to find at length that it was the victim of misplaced confidence was indeed a cruel fate.

9. But our study of the case did not leave the issue simply with the conviction that the rim of the nebula would separate continuously into a disk; it went farther and raised two much more radical questions, the first, whether the passing off of the molecules individually would not *forestall* the state at which the centripetal force of gravity would be overtaken by the centrifugal force of rotation; the second, whether the molecular orbits would really be circular, as assumed, or whether on the contrary, they would not be so far elliptical as to vitiate the reasoning by which the rings were regarded as logically necessary.

In the first chapter, the way in which the common collisional atmosphere passes into an ultra-atmosphere of vaulting molecules, and the way in which a part of these vaulting molecules pass into an ultra-atmosphere of molecules in orbital flights, were set forth. Now it is clear that centrifugal action aids the passage of the collisional molecules into vaulting molecules and also aids the passage of some of these vaulting molecules into orbital molecules. Every increase of rotation, by increasing the centrifugal tendency, increases the transfer of molecules from the collisional to the vaulting and from the vaulting to the orbital states. This transfer is at the expense of the momentum of the rotating body, for the orbital molecules require a higher mean value of momentum than the mean value of the momentum of the molecules of the collisional atmosphere. If the process is closely followed, it will be seen that as the centrifugal tendency increases almost to equality with the opposing centripetal force of gravity, the number of molecules that are driven by collisions into vaulting leaps and orbits is increased. The momentum requisite for the orbital movements is taken from the molecules from which the last leaps were taken. The loss of these molecules is equated later with the rest of the nebula. The inference from this is that increase of rotation, in such a body, necessarily finds issue in increasing the number of molecules that pass into orbits, and that the nebula, because of its constant loss of momentum, would never reach the state at which molecules will be separated *simply* by centrifugal action; they would rather be separated by *molecular activity* superposed on the high rate of rotation attained.[5] If this seems a too subtle distinction, it is to be observed that the molecules which go off through molecular activity pursue orbits that have a great variety of eccentricities so that their subsequent aggregation into planets and satellites is conditioned by these eccentric orbits and is not amenable to the logical deduction relative to rotation cited above as playing so important a part in cosmogonic thinking for the past century. Reference must be made to a later discussion on rotation for the full meaning of the distinction between aggregation from circular concentric orbits and aggregation from heterogeneous elliptical orbits respectively.

10. Moulton has shown that, even if a ring were formed, the breaking of this ring at some weak point and the collection of the whole into a globe, as postulated in the Laplacian hypothesis, if it does not traverse the laws of celestial mechanics, is at least attended with grave difficulties. Even if a large nucleus were formed at some point on the ring to serve as a collecting center, it probably could not gather to itself bodies in independent circular orbits from an angular distance of more than 60° without the co-operation of some other agency. The considerations in this case are too technical to be introduced here. So also are some other criteria developed by Dr. Moulton in the course of his inquiry.

If then the probabilities are strongly against the formation of a coherent ring by centrifugal action, and if such a ring, granted that it be formed, could not hold the lighter molecules at the postulated temperatures in such a case as that of the earth, and if, in addition, the mechanical difficulties of segregation into a single spheroid were highly adverse, if not insurmountable, even under the most favorable circumstances, this line of genesis offers little in its favor to offset the grave incongruities and discrepancies disclosed in the mechanics of the system.

It would, however, no doubt leave an unfair impression of the hypothesis of Laplace, and of its unsurpassed simplicity and beauty, and of the great service it has rendered the progress of thought, if there were no recognition of the fact that there is a long list of general harmonies between the salient features of the solar system and the broader terms of the hypothesis. On such general harmonies the hypothesis was founded, and from these, it gathered to itself a wide adherence. All this was meritorious in its day. It was only by the progress of discovery—to which, indeed, it had itself made noble contributions—and by advance in analytical inquiry, that these broader harmonies were found to be merely general, while specific incongruities of a grave nature were disclosed. In some notable measure, though not wholly, these incongruities were veiled at the time the hypothesis was given to the world by the great French astronomer and mathematician.

Before passing to the next phase of our inquiry, a word is to be said relative to some of the cosmogonies that preceded the admirably specific theory of Laplace. Only general reference has been made to these thus far for they really took almost no part in the inquiry. There were specific reasons for this. For the greater part, they had not been worked out into specific details that could be applied closely to the peculiar dynamic features of the earth and its planetary kin, and, for this reason, they were not fitted to play any serious part in an inquiry that tried to proceed naturalistically on the specific testimony of the dynamic vestiges borne by the planets. It was of some interest, to be sure, that these general cosmogonic theories were more or less susceptible of being made the point of departure for some new view of the genesis of the earth that was specific, if one felt that the facts of the dynamic record made such an effort promising. But the views actually offered for consideration were, in general, too vague to take their places beside the clear and sharp tenets of the Laplacian hypothesis. As earlier intimated, the

Laplacian hypothesis stands alone among the older views in its laudable definiteness.

THE LESS SPECIFIC HYPOTHESES

To a notable extent, though not universally, the other older cosmogonic theories centered on the profound problem of original creation, or, at least, on the primitive state of celestial matter. Following back along the line of terrestrial evidence, as our inquiry did, and clinging as closely as possible to the evidence of the earth's automatic record, it would have been an unwarranted leap into the depths of speculative assumption to have presumed that, when we had reached the stage of planetary genesis, we had also reached the beginning of things. Nothing whatever had been found in the record to imply that the birth of the earth was a feature of the absolute beginning of the universe. As intimated already, once and again, there seemed no ground to assume that the origin of the earth stood as the only type of origin of secondaries in the great universe, or that it was a part of primitive creation, however naturally it may have been assumed by the ancients to be a creative *ultima Thule.* There had not even appeared in the record any clear evidence that there was a primitive creation *ex nihilo*, as distinguished from an indefinite backward extension of cycles of celestial evolution. The trend of our inquiry—a trend that will appear even more distinctly in its later stages—lay rather in the latter direction. The inquiry had been leading—and it continued to lead—step by step to the impression that the creation of our planetary system was but an incident in the history of our sun, while even the genesis of the sun might not improbably be but an incident in the history of our stellar galaxy, and the genesis of that perhaps only an episode in the evolution of the real universe that undoubtedly lies chiefly beyond our ken. It appeared, therefore, that, while our inquiry might lead on ultimately to some consideration of the evolution of our stellar galaxy, and to the modes of genesis of the stars and their attendants—which, springing from surpassingly rich resources of energy and activity, might follow many different lines—only a small part of this broad complex problem lay within the specific field of our inquiry. This fraction, however small relative to the whole, was none the less all too great for our investigative resources.

To a large extent, as intimated, the older cosmogonic views centered upon the speculative concept of primeval chaos, or a modified view of it, and made this chaos the initial stage of stellar history. Under this assumption there usually lay the postulate of absolute creation, but this was not always the case. Creation *ex nihilo* was accompanied, or followed, putatively, by endowments of energy, activity, and the various properties of matter, and these led on to a series of events which brought the sun and the planets into being. The whole solar system was thus made the direct offspring of a primitive series of events. The sun and the planets were assigned a common birth. The details and special stages were more or less successive, indeed, but the whole was one great unitary evolution. This general postulate of a common, and essentially contemporaneous, evolution of sun, stars, planets, and satellites was a feature common to most of the older cosmogonic theories. In this they were at one with the Laplacian hypothesis. A departure from this prevailing conception appeared, however, in the latter part of the eighteenth century, in the collisional hypothesis of Buffon. To this we shall refer later.

Where primeval chaos, or any form of a universal diffuse condition was taken as the original state, some form of segmentation must necessarily have followed as a means by which appropriate volumes of the diffuse matter came to be separated from the rest in a form suited to gather later into the several stellar systems—the solar system being the case of particular interest to us. The mechanics assigned for such a segregation were very obscure, or altogether neglected. Even the segregation itself was passed over lightly. If original uniformity was assumed, as obscurely implied in most cases, as definitely stated in some, the assigned agencies that actuated such sub-segregation rested on doubtful bases. If in any case departure from original uniformity was implied, no specific asymmetry suited to produce just the right kind of segregation seems to have been postulated in any instance. Such dynamic insight was perhaps more than could be expected from the attainments of the earlier ages in mega-mechanics.

Passing this by, the dynamics of the later processes by which the subdivisions of the universal chaos gathered into stars and planets were often scarcely less obscure. When definite, they were usually untenable.

Aside from the great historical interest that attaches to these earlier attempts at the solution of the great problem of the genesis of the heavens and the earth, and apart from the genuine admiration they awaken for the ingenuity and breadth of view some of them display, notwithstanding their shortcomings, only one among them has claimed the serious attention of modern scholars, the Kantian hypothesis, and that not very widely. If our inquiry had been a study in general cosmogony, instead of a search for the genesis of our planet, the Kantian view might perhaps have had certain claims to take precedence over even the Laplacian hypothesis, for the latter purposely stopped short of a comprehensive philosophical view of celestial evolution; it neglected to assign an origin, or to delineate the early history, of the nebula with which it dealt; it started with an assumption; it simply postulated a nebula of given mass and physical state; it did not, even by speculative hypothesis, connect this nebula with its own origin or antecedent history, much less with an absolute beginning, or even with a general parental state, as did the Kantian hypothesis.

Unfortunately the Kantian hypothesis was made to rest on the untenable view that the rotatory momentum of the system would arise inevitably from the centripetal action induced by gravity and the reaction of atomic repellency. Other mechanical infelicities crept in also. These inhibited any modern building on the Kantian basis, unless its dynamic foundations were replaced by sound tenets, and tenable substitutes did not offer themselves that were not, in essence, abandonments of the Kantian concept. It has been seen that the rotatory momentum of the planetary system is not only a very radical, but a very discriminative, element in the dynamic constitution of the solar system. It has already appeared—and the observation will gain in force as study proceeds—that the dynamic endowments of the sun, on the one side, and of the planets, on the other, are in such striking contrast that they seem to imply that these two sections of the solar system had different histories, or, at least, that some differentiating agency entered into their histories in such a way as to give them contrasted and incongruous endowments of momentum. The critical considerations that grew out of a study of these differential endowments, far from leading back toward a simple evolution from a common apportionment of primitive chaos, seemed to point quite specifically in the opposite direction. The observed apportionments of mass and momentum in the solar system were found to depart widely from the apportionments naturally assignable to a systematic mode of evolution from a single common mass segregated from the primitive chaotic universe. The Kantian hypothesis seemed, therefore, so completely excluded, not only by the fallacious mechanical concept on which it was based, but also by its evolutionary unfitness to meet the requirements of the case, that it held out no inducement to serious consideration. Even if its mechanistic infelicities could be replaced by sound ones, it was obvious that it would encounter at once the more serious of the difficulties that were found to bar out the Laplacian hypothesis. Whatever place, therefore, the Kantian views may be entitled to hold in general cosmogony, they did not seem, while our inquiry was in its early stages—still less do they now seem—to have any serious claims to consideration as an account of the way the earth and its fellow-planets came to be what they are.

THE CRITICAL FEATURES OF OUR PLANETARY SYSTEM

It is pleasant now to turn from the unwelcome task of destructive criticism, however unavoidable, to the story of constructive effort, to an endeavor to find the physical forebears of those dynamic features of our planetary system that had been gathering sharpness of significance as the inspection incident to these criticisms proceeded. Let us review them hastily then before turning to the constructive efforts to which they led.

1. It is to be noted with emphasis that our planetary system is a closely appressed disk of revolving bodies centered on an invariable plane; and that the total mass of the planets is very small ($\frac{1}{745}$) relative to the mass of the sun. If the sun and the planets are the divided parts of a common nebula, the process of partition must have been such as to realize this very unequal division in this very specific form. Such an extreme inequality of partition seems improbable from any centrifugal or other agency acting proportionately on a common mass. The extreme flatness of the discoidal form points to some powerful genetic agency competent to enforce upon the system the appressed configuration it still bears. This might well have proved one of the strong points of the Laplacian hypothesis and of the whole centrifugal genus, if the specific details had not been found so seriously adverse; for a swiftly rotating attenuated mass should give a narrow discoidal configuration to a planetary system derived in this way. Heterogeneous assemblages of scattered matter coming in from various directions, however, seem to be still more effectively barred out because they appear to be

unsuited to give this markedly discoidal configuration, and this singular partition of material.

2. While the discoidal form shuts out a large group of hypotheses, the departures from perfect symmetry in the masses and in the arrangements of the planets are no less critical. While the orbits of the planets are subcircular, they are yet notably eccentric, varying from 0.00684 to 0.20560; while the eccentricities of the orbits of the planetoids vary from 0.07631 to 0.2228. The inclinations of the planes of the planets to the plane of the earth's orbit vary from above 7° downward. The orbital planes of the planetoids are notably more inclined, ranging up to 38°. While the limitations of these variations betray the general control of some powerful genetic agency that made for a discoidal form, they show equally that the control was neither wholly complete nor strictly unified. The variations imply the influences of deviating agencies, but only agencies of such minor efficiency that they could merely superpose small divergencies upon the symmetry induced by the master force.

3. Not only are all the planes of the orbits of the planets and planetoids inclined to the equator of the sun, but the invariable plane of the planetary system, a dynamic summation of the planes of the whole planetary group, is inclined to the plane of rotation of the sun, though the sun is the controlling body of the system in a gravitative sense. This inclination is not great enough in itself to be very impressive, but it falls in with the other vestiges of deviating influence and adds to their significance. The inclination of the sun's plane of rotation gains significance when it is recalled that it affects 744 of the 745 parts of the mass of the system, while

the several divergent planes of the planets taken all together affect only one of the 745 parts. The significance is not so much in the fact that there is variation in the inclination of the planes of the individual planets from that of the controlling body, as that their composite value, represented by the invariable plane of the planetary system, is inclined.

4. The central controlling body, though it carries $\frac{744}{745}$ of the mass, carries less than 2 per cent of the revolutionary momentum of the system. The remaining $\frac{1}{745}$ of the mass, in the form of the planets and the satellites, carries above 98 per cent of the momentum.* This disparity, so deadly, it would seem, to all centrifugal theories of genesis, must probably prove deadly to some or all of such other theories as are built on an unsound dynamical basis, while it should help to point the way to a tenable theory. It is a severe criterion; its application may be expected to narrow greatly the range of permissible theories.

5. The directions of rotation and revolution, which, when the fathers of cosmogonic effort gave forth their hypotheses, were all of one sense, so far as then known, have been found, as discovery has proceeded, increasingly divergent, discordant, and puzzling. This recently reached an extraordinary climax in the discovery that, while the majority of the moons of Jupiter and Saturn revolve in harmony with their primaries, a small minority, two in the Jovian family, one in the Saturnian, revolve in the opposite direction. This is a climacteric anomaly.

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*The momenta of the planetoids are not here included; they would somewhat increase the disparity.

All of the criteria involved in these singular features must be met by any hypothesis that is entitled to be regarded as having even working qualities; they must, of course, be fully met by the true theory, as well as critical features that have not been cited. The list here noted falls short of being exhaustive, but even these bring into view a series of criteria whose severe requirements are at once formidable and directive.

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CHAPTER IV

FUTILE EFFORTS

When some fundamental faith on which one has long rested gives way beneath him and he finds himself plunged in a sea of doubt, it is in the natural order of things that he should flounder awhile in the endeavor to find a new bottom or a new float. His stress is all the more keen if he awakens at the same time to a realization that unconsciously he has built freely upon his faith and that many a pet edifice must go to wreck if the foundation is really gone. No one quite realizes how much of accepted doctrines, of current interpretations, and of working assumptions have been built subconsciously upon the nebular hypothesis and upon the derivative doctrine of a gaseo-molten earth. No small part of the traditional tenets of geology are imperiled if the gaseo-molten state of the primitive earth is really brought into question.

While there was only partial appreciation of this at the time, it was felt to a disturbing degree. At first, however, there was some comfort in the feeling that there were many alternatives upon which it was easy to fall back if the old view really proved untenable. If the earth did not arise from a gaseous nebula that shed rings to form planets, it seemed a light matter to shift belief to an origin from a meteoritic swarm that masqueraded as a nebula, or to an aggregation of meteorites gathering in from the four quarters of the heavens. Perhaps stars might collide and reform into a new system, or nebulae might encounter one another and start a special evolution, or some other of the possible permutations and combinations of celestial agencies might have functioned in a genetic way. With such a plethora of alternatives it was easy to persuade one's self that our geologic tenets might be transferred to some new cosmogonic base and still be entertained, if the old foundation could carry them no longer.

And yet the question continued to rise insistently: How many of these alternative concepts had really escaped the stress which dynamical laws appeared to put on the most symmetrical and complete of all the inherited hypotheses, not to say the hypothesis most honored by an eminent parentage and a noble clientèle. Were the vague alternatives any better grounded than the definite and beautiful hypothesis of Laplace? Obviously there was no logical resting-place for one's confidence short of some definite foundation that would stand the searching tests of dynamics, or at least seem to do so. It was idle to say that we can proceed without basal concepts; if they are not consciously adopted, they unconsciously insinuate themselves and thus take on their most deceitful forms. The unctuous feeling that one is dealing "only in solid facts" is too often merely a tenuous cover for subconscious speculations that swarm in the turbid substratum of thought. It is easy of course to be content with the protruding elements of thought and to neglect the assumptions on which they unwittingly float; it is easy to be quite sure of the obtrusive products and quite unconscious of the assumptions and speculations which may be their only buoy. The nearest approach to security that can be attained lies in tracing all tenets back as far as possible, with critical examination of their grounds, yielding assent to them only in proportion as they link themselves with the best established principles that condition natural phenomena.

INQUIRY ALONG OTHER GASEOUS LINES

In the attempt to find a tenable view of the origin of our planetary system—an attempt which naturally followed loss of faith in the Laplacian and related hypotheses—no success was had along gaseous lines. The tests based on the kinetic theory of gases and on the laws of dynamics seemed to cut a deadly swath through all assignable outgrowths of gaseous states *so far as these have had to do with our own planetary system.* I beg that this distinction between *our* planetary system and other *possible*, and probably actual, planetary systems be kept clearly in mind. There is no question that a certain group of the nebulae are gaseous, and there is good ground to believe that such nebulae develop secondary systems—planetary, planetoidal, or otherwise—along consistent gaseous lines; but the evidence brought out by the previous inquiry seemed to leave no ground to believe that *our* planetary system arose in this way. All efforts in gaseous lines appeared thus not only to be futile but there seemed to be no encouragement for further efforts with any medium over which laws of the collision-rebound type presided.

INQUIRY ALONG METEORITIC LINES

To this last category belong all meteoritic hypotheses of the quasi-gaseous type, that is, all meteoritic hypotheses in which the deployment of the meteoric swarm is supposed to be maintained by collisions and rebounds of the constituent meteorites. Sir George Darwin has shown, in a masterly mathematical study, that if meteorites are assembled so as to collide and rebound in a miscellaneous way as do the molecules of gases, and if, in such collisions, essentially perfect elasticity is brought into play by the generation of gas at the points of impact and the instant expansion of this gas, the whole assemblage will follow the laws of a gaseous body; the meteorites, at least up to sizes comparable to the cannon balls of former days, may be treated as gigantic molecules.[1] The uncertain point in this deduction lies in the doubt whether meteorites in collision would, as a matter of fact, develop the elastic recoil essential to the validity of the conclusion. If not, the collapse of the assemblage would apparently be more rapid than that of a true gaseous body and the evolution of heat would be faster; the liability to pass into a true gaseous condition would thus be imminent.

But, whichever alternative obtained, the behavior of a quasi-gaseous swarm of meteorites would follow the same general course as the evolution of a gaseous nebula. In neither of these meteoritic alternatives should the swarm normally have a larger ratio of centrifugal momentum to mass than does a gas—nor should the momentum be better distributed. Now Moulton's trenchant studies have shown that a gas, normally distributed according to the law of gases, does not carry enough moment of momentum, nor the right distribution of moment of momentum, to develop into such a planetary system as ours. At first sight, the quasi-gaseous form of meteoritic hypothesis has its attractive features, but on closer scrutiny it appears that it is not more promising than the true gaseous hypotheses, if indeed it is not in some respects less promising, even if we ignore all grounds of doubt as to the reality of such a nebular constitution.

There are, however, other meteoritic hypotheses. The most strictly meteoritic of them all is that old view which took its cue from the observed habit of the meteorites that nightly illumine our present skies. From this it reasoned backward in logical consistency. It perhaps alone is meteoritic, in the strictest sense, in that it deals with true meteorites actuated by the demonstrable habits of meteorites. The reasoning runs as follows: The earth now gathers in meteorites daily by millions; there must be just so many millions less in open space today than there were yesterday; there must have been millions more at each earlier interval than in each later one; more were picked up daily in early times than now; in the very early days, the accretion was very rapid and the growth fast. This is logical thus far; but the hypothesis halts or grows vague just when it should press on sharply to the initial point, the point on which everything hangs. Up to its halt the working basis is the *pre-existence* of the earth and its service as a collecting center. The hypothesis spends its force upon this source of unquestioned growth while it fails to point out the origin of the mechanism of which this growth is an incident. When scrutinized relative to the essential initial condition, it seems specially incompetent, for meteorites are seen to be plunging through space with various velocities in various directions and in a very sporadic way, except as they are the relics of dispersed comets which are themselves scarcely less erratic. The velocities of the meteorites are so various and high as to imply a dispersive rather than a segregative tendency. They offer no suggestion as to how a planetary nucleus could spring from them. Their momentum is so vastly superior to their gravitative power that the conditions they offer appear to be distinctly inimical to segregation except as they are caught by some masterful body.

Even if this fundamental difficulty could be avoided, it does not appear that there is any systematic preponderance of infall from any one direction, except as local showers arise from dispersed comets, and even these are as heterogeneously disposed as were their parent comets. But a marked preponderance is prerequisite to the disk-like arrangement of the planetary revolutions, one of the most pronounced characteristics of the system.

At best then this line of search merely leads back to the vital questions: What gave origin to the planetary *centers of collection?* What made the planets revolve nearly in the same plane? Efforts along this meteoritic line—the true meteoritic line—seem therefore futile, if the search is for the *origin* of the planetary system. This meteoritic hypothesis merely lays emphasis on a mode of growth—undue emphasis, it would appear, on what is probably a merely incidental rather than an essential mode of growth. Meteoritic growth at present is so extremely small as to be practically negligible, as shown by Woodward and others. There is little or no specific support for any presumption that such meteoritic growth ever rose in geologic time to appreciable quantitative value.

The naturalistic mode of testing this hypothesis, by an appeal to the vestiges of former conditions presented by the meteorites themselves, and the significant features of their singular structures, is of the same general import. Meteorites have rather the characteristics of the wreckage of some earlier organization than of the parentage of our planetary system.

In a third class of views grouped as meteoritic, in the loose sense of that term, nebulae are interpreted as assemblages of meteorites pursuing orbits about a center of gravity. The orbital feature of the hypothesis carries the case over into quite another field of dynamics, orbital dynamics, and a prompt growl of protest from the traditional "lion in the way" is naturally provoked. It was currently held during the last century that the whole field of orbital evolution was effectually barred by the deduction that retrograde rotations would result from the aggregation of bodies in orbital revolution, because as a matter of fact the larger number of rotations in our planetary system are coincident with the revolutions. So long as this "lion in the way" of cosmogonic adventurers was held to be living and real, it was of course idle to seek an origin of our planetary system by an evolution from meteorites in orbital revolution around a common center.

But, neglecting this traditional difficulty for the time being, certain other limitations affecting evolution under orbital dynamics are to be taken into serious consideration. The cooling and shrinking of celestial bodies have no effect on their orbital motions, and hence cooling and shrinkage, which play so large a part in familiar cosmogonic hypotheses, have no vital function in evolution under orbital conditions. Under such conditions the vital issue concerns collecting centers and conjunctions of orbits. If small bodies pursuing individual courses cross the orbits of adequate collecting centers, they furnish the mechanism of growth. Without adequate collecting centers such crossings are more likely to promote fragmentation and dispersion than aggregation.

If the orbits of the small bodies to be gathered into planets strike in all directions promiscuously—are quaquaversal as the geologist would say—it is clear that the case is unpromising for the evolution of planetary disk such as the actual case requires. The concentration of such a quaquaversal system tends toward a globular aggregate in which heterogeneous collisions prevail. More or less concurrent orbital revolutions are required to give rise to a concentric harmonious organization of discoidal form. Concentration in a quaquaversal case really trends in the direction of a gaseous body and in most cases of notable mass and complexity, any close concentration of the assemblage would, with little doubt, result in a passage into an actual gaseous aggregate.

In the vital matter of momentum, the tendency in such a case is toward a low value, since revolutions in opposite directions more or less offset one another in aggregation and only the algebraic sum of the individual momenta remains. Considered thus from the critical point of view of momentum values, the case is unpromising. Apparently the only line of escape from this untoward trend lies in postulating a distinct preponderance of revolutionary values in one direction over those in all other directions, which, in reality, is the abandonment of the case and the substitution of a new case, that of orbital revolutions dominantly of the same phase.

FIG. 1.—The spiral nebula M 74 Piscium, a symmetrical form, whose two arms bear a notable series of knots that seem admirably adapted to be collecting centers for the adjacent nebulous matter. Photographed at the Lick Observatory.

This substitute offers a nearer approach to working conditions. It seems at least to lie in the right direction, if only the rotational bar is not prohibitive. When, however, constructive effort is pursued along this line, it becomes at once obligatory to point out or to postulate —with reasons therefor—the necessary collecting centers in proper number and relations—four powerful centers to gather in the orbital matter to form the four great planets; four centers of medium efficiency to collect matter for the four minor planets; a multitude of small centers to grow into the hundreds of planetoids, and withal several small groups of secondary centers revolving around the planetary centers to segregate into satellites. Matter in orbital motion does not aggregate spontaneously in the simple fashion that obtains with static matter. The required nuclei of aggregation do not seem to be natural elements in a simple spheroidal or discoidal nebula where each small body is pursuing an orbit of its own. Observational evidences of nuclei, or cogent dynamical reasons for their origination, do not seem to be at hand.

Such centers of aggregation are, however, marked features of spiral nebulae, whose arms are singularly affected by knots which, in the nature of the case, may be confidently assumed to function as collecting centers. These spiral nebulae, however, constitute a class quite distinct from spheroidal aggregates and claim attention on their own grounds. It is interesting here to note, however, that, in so far as spheroidal nebulae are constructively modified to take on promising qualities, they approach the characters possessed in a more eminent degree by spiral nebulae (see Fig. 1).

INQUIRY ALONG COLLISIONAL LINES

More than a century ago the naturalist Buffon advanced the theory that our planetary system arose from the collision of a great comet with the sun. While later knowledge of comets has rendered this view quite untenable, Buffon gave definite initiation to the collisional genus of cosmogonic hypotheses. Collisional views of genesis of various less obviously untenable types have been entertained since. Without doubt the possibilities of collision are entitled to a place among cosmogonic studies, for encounters undoubtedly occur and almost inevitably they must be followed by some form of reorganization or recollection of the scattered matter, though this does not necessarily, nor perhaps generally, imply a reunion into a single body. The question in hand, however, is the narrower one: Did any form of collision initiate the conditions out of which our planetary system arose? The verity of the genus is not proof of the species, much less of the individual case. Here, as before, the decisive criteria are to be sought in such vestiges of its earlier stages as are still borne by our planetary family.

The conditions to be met are definitely fixed; the result of the collision must yield a central mass of the magnitude of our sun; this must be surrounded at once or ultimately by eight rather large masses and a multitude of small masses, all in subcircular revolutions. These smaller masses must together equal about $\frac{1}{745}$ of the total mass. This small factor must carry 98 per cent of the moment of momentum of the whole system.

Now center-to-center collisions, or anything approaching such collisions, seem to be excluded by these conditions, for if the impinging body were small, it would be simply swallowed up in the great solar mass; if it were sufficient in mass or in velocity completely to traverse the solar body, the result probably would partake of the nature of a vortex of the smoke-ring type. The ring nebulae may possibly fulfil the requirements of such a case, though other possible interpretations of these singular objects may be entertained (Fig. 2). Extreme dissociation no doubt would follow such a

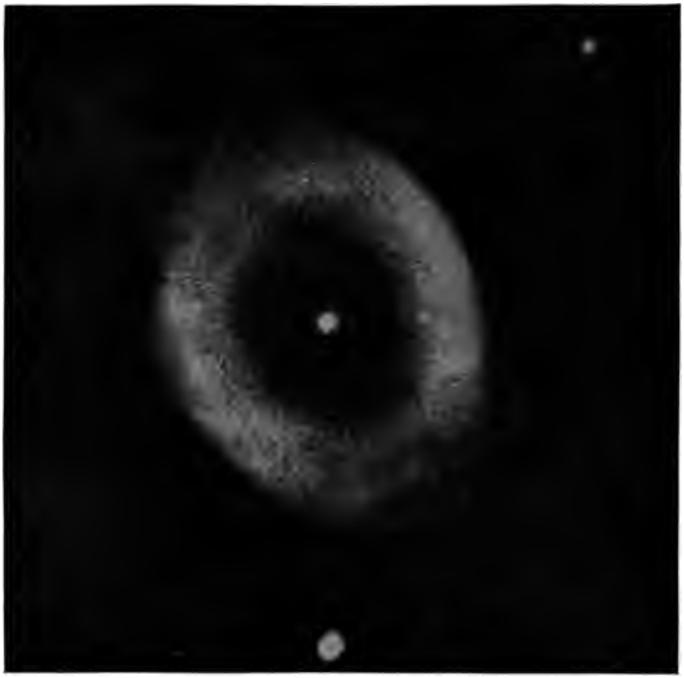

FIG. 2.—The ring nebula in the constellation Lyra. Photographed at the Lick Observatory.

piercing stroke, since the velocity of collision would be high if the mass of an average star were involved. The spectroscopic natures of the ring nebulae tally with this presumption.

It is difficult to imagine a case of head-on collision that could leave its wreckage in a state suitable for gathering into a system like our own, for radial dispersion is the normal result. The only case of promise is a glancing collision; that at first seemed to be quite hopeful and was industriously tried.

In cases of glancing collision certain conditioning features are inevitable and need to be taken seriously into account. The course of the impinging body at the time of impact is a sharp curve, not a straight line as sometimes pictured. This curvature is further conditioned by a severe tidal strain due to the differential attraction of the two bodies then very close together, and this alone involves danger of disruption, if not of violent projection, even before collision takes place. If the impinging body is affected by high internal elastic compression, expansion enters also into the combination of conditions, since gravity is neutralized on certain lines and supplemented on others by compression and this adds to the disruptive and dispersive tendencies. Only in an exceptional case, if at all, is it safe to assume that an impinging body at the instant it nears collision with a massive body of the order of the sun can maintain its integrity under the disrupting influences. If it is gaseous at the start, it must yield freely to the dispersive tendencies.

The velocities at which collisions would normally take place are forbiddingly high, and excessive dispersion becomes almost inevitable. The case in hand requires, as its minimum, the mass of the sun plus the mass of the planets. A planetary body merely falling under gravity from some point outside the sun's normal sphere of control would have a velocity of the order of three hundred and eighty miles per second at the instant of collision. It is possible to escape some of this troublesome velocity by assuming that the solar body was in an expanded condition. A glancing impact would then take place at a greater distance from the center of gravity and the velocity would be correspondingly lower. It is scarcely permissible, however, to assume that the expansion reached the orbit of the innermost planet about to be formed, for that would prevent the deployment of that planet. And so the velocity of collision could not well be reduced below fifty miles per second by postulating expansion. Even this cannot be done without incurring some incidental difficulties, for the outer border of so expanded a sun would be very attenuated and perhaps formed chiefly of the lightest gases, which would not be felicitous material for forming the earth. Perhaps, however, this might be assigned to the colliding body.

It is difficult to picture the effects of collisions of velocities ranging from fifty to three hundred and eighty miles per second. Extreme dispersion would seem to be inevitable, perhaps even atomic dissociation. The line of dispersion should radiate from the point of impact, and one can scarcely imagine the formation of aggregates to serve as centers for the collection of planets in the dispersed matter. As only a portion of one or both bodies is supposed to be in direct collision, the rest might limit the dispersion in its direction and give it the semi-radial form seen in several cases in the heavens, of which the two great nebulae of Orion are the most notable examples (Fig. 3). These may possibly have been co-partners in a mutual collision.

FIG. 3.—The Great Nebula in Orion and the Fish-Mouth Nebula. Photographed at the Yerkes Observatory.

The intensity of dispersion and its divergent radial nature are serious difficulties in the way of forming a plausible hypothesis of the formation of a planetary system such as ours as the sequel of a glancing collision. Before serious inspection, the case of a small meteoroidal nebula driving nearly tangentially into the very attenuated border of a large solar nebula seemed to me to present a hopeful basis for such a hypothesis, and considerable constructive effort was spent in the endeavor to find consistent working conditions that might eventuate in our planetary system, but in addition to the infelicities of dispersion arising from high velocities of collision, other formidable obstacles arose.

It is a law of celestial mechanics that bodies thrown into orbital paths by encounters must, in completing their courses, return to the point of collision, which in this case would be the edge of the sun, or of the body that was to form the sun. Of course bodies might be driven off so violently as to fly beyond the sphere of control of the solar mass and be irrecoverable, and this would be a rather imminent contingency, but all such dispersed matter as remained under solar control—and this of course included all that could enter into the formation of planets—was compelled to come back to the point of collision and be subject to renewed collision, and so on indefinitely. Some partial escape from these fatal conditions might arise in the case of such molecules or other highly elastic bodies as experienced secondary collisions in the course of their flights and, by reaction from these, established new orbits, with a necessity of returning to the point of this secondary collision where the chance of a new encounter might be lessened.

There might also be some escape when the attraction of the impinging body, after collision, drew the flying matter into new orbits, if the impinging body remained in a sufficiently aggregated state to exert any appreciable centralized attraction. The effectiveness of either or of both of these diversions working together is very doubtful. In any case the new orbits were likely to remain very eccentric and to have their perihelion points near the sun.

There are ways in which eccentric orbits may be reduced to subcircularity, but the extreme eccentricity of the orbits that would arise from collision, and the difficulty of finding any applicable and adequate agency for the reduction of these orbits to the requisite subcircular form of the present planetary orbits, and for giving them the spacing of the existing system, seemed to be so far insuperable that constructive effort in this line was abandoned.[2]

This disappointing outcome, however, had a directive effect. As in the inquiry on meteoroidal lines, the results suggested the general direction in which a successful hypothesis must probably lie. Even more directly than in the meteoroidal case, they pointed to spiral nebulae as promising forms. Celestial collisions were indeed one of the sources to which the origin of spiral nebulae was then referred. While such a genesis of spiral nebulae could scarcely be regarded as supported by the considerations we have just reviewed, some related source might perhaps be found to fit both the origin of the spirals and the genesis of our planetary system. The supreme weakness of referring spiral nebulae to eccentric collisions lies in the fact that such an origin implies *a single spiral arm* or set of arms, springing from the side of the nucleus at which the collision took place, whereas spiral nebulae habitually show *two* arms or sets of arms arising from diametrically opposite sides of the central mass. Some other source for spiral nebulae, and for our planetary system alike, seemed to be indicated by the study of collisional effects. But the "lion" was in the way here. The deployment of spiral nebulae makes it extremely improbable that their arms, their knots, and their scattered matter have any material support such as the resting of the outer parts on the inner parts, as in the case of gaseous bodies. Each part must obviously move in an independent path and be supported by its own moving force. Thus these bodies, above perhaps any other nebulous form in the heavens, lay under the rotational ban. All such bodies, it was said, should have retrograde rotations, whereas most of the planets have forward rotations. If this "lion in the way" was a real lion, it was idle to waste time on spiral nebulae, unless it could be shown that the lion was chained to some special case or condition which the creative process had in some way avoided in forming our system.

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CHAPTER V

THE FORBIDDEN FIELD

As already noted once and again, there seemed to be a lion in the way of entering the field of orbital organization in search of the origin of our planets—an adverse argument, a hostile demonstration, so clear and cogent that it stood as a prohibitive ban. It ran in this wise: In every body rotating as a spheroid, or as a disk, or as a coherent ring, the outer part moves faster than the inner part, and hence if a ring, or any symmetrical section, separates and condenses, the faster outer parts swing forward around the slower inner parts and the rotation is forward in all normal cases. The argument is made all the more conclusive if it is observed that, even before separation and condensation, the outer part was swinging round the inner part once with every rotation of the whole. The part in question thus already had a forward rotation, and this rotation would of course be accelerated by any contraction that might follow (Fig. 4). On the other hand, if the spheroid, disk, or ring were formed of discrete particles, each revolving in its own independent orbit, the inner particles, as a necessity of the orbital state, must move faster than the outer ones, and so, when any symmetrical section of these independent bodies collects into a single mass, the higher velocity of the inner bodies imparts a retrograde rotation to the conjoined mass (Fig. 5). The reasoning seems irreproachable.

But is this second case really representative? Does it even belong to the type of organization we wish to put to trial as a possible source of origin of our planets?

It is clear that the case was fashioned from the picture of Laplacian rings, Saturnian rings, and similar symmetrical forms in which a very regular concentric arrangement of circular orbits obtains. In these cases it was tacitly assumed that, in collecting, the orbits would remain circular and the concentric arrangement continue to prevail. Without consciously specifying it, the collecting process appears to have been assumed to arise from a symmetrical enlargement of the inner orbits, or a symmetrical shrinkage of the outer orbits, or both together, with no essential alteration of their concentric relations or their circular forms. And yet it is clear that if the orbits were so slightly disturbed or distorted that a faster-moving inner body should swing out only so far as to strike a neighboring body on its outer half rather than its inner half, the two bodies would rotate forward. This illustration, to be sure, involves a special assumption that is perhaps not normal to the case, but it serves to show how slight a variation of conditions reverses the results.

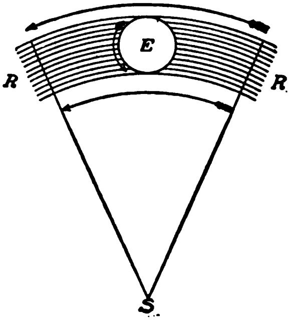

FIG. 4.—*RR* represents a ring of gas moving as a unit and hence the outer portion the faster. If converted into a spheroid, *E*, centrally located, the rotation is forward, as shown by the arrow.

Now in a spiral nebula there is no reason to suppose that the knots, or the particles of the haze, revolve about the center of gravity of the nebula in orbits that are either strictly circular or closely concentric with one another. On the contrary, there is reason to suppose that these orbits are rather notably elliptical and rather diversely related to one another, so that they cross in various ways and at different angles, though in a general way, fairly concordant. Something of this sort would undoubtedly be true of orbits arising from collisions—and probably also of meteoroidal orbits. Observation and theoretical inference alike imply that circular orbits are the exception in the heavens rather than the rule. Even in a sub-circular system like our own, the ellipticities of the orbits are far too great to make it safe to predict the precise way in which one planet would strike another, if their orbits were so shifted as to make a collision possible. Before the results of a conjunction of revolving bodies can be treated safely, it is necessary to consider the specific modes in which conjunctions may occur and the velocities that obtain at the instant of collision.

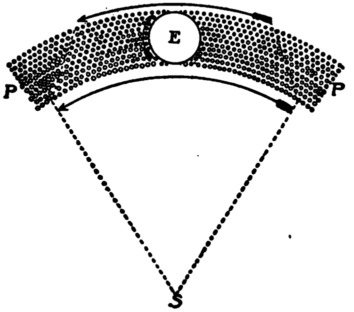

FIG. 5.—*PP* represents a belt of planetesimals revolving concentrically about the center, *S*. If these collect about the central point of the belt into a spheroid, *E*, by the enlargement of the inner orbits or the reduction of the outer ones, the concentric arrangement remaining, the rotation will be retrograde, as shown by the arrow.

In the first place, the movement of a body in an elliptical orbit, instead of being uniform—as is the case of a body in a perfectly circular orbit—varies from a maximum speed when nearest the controlling body, to a minimum speed when farthest from it. In the second place, conjunction in elliptical orbits of different types can take place at certain points only. A planetary body moving in an outer elliptical orbit can come into collision with a body in an inner elliptical orbit only when some part of the outer swing of the inner orbit (the aphelion portion) coincides with some part of the inner swing of the outer orbit (the perihelion portion). The whole case of rotatory effects hangs on the relative motions of the two bodies in *these portions* of their orbits irrespective of their mean velocities in their whole orbits. The simplest case is that in which the aphelion point of of the inner orbit coincides with the perihelion point of the outer orbit. This point is then the *only* one at which the bodies in the two orbits can come together. Two instances of this type are illustrated in Fig. 6. The relative velocities in such cases are of course mathematically determinable, but, without computation, it is easy to see the essential fact by simple inspection. Starting at either point of contact it may be seen that the inner body lacks sufficient velocity to maintain its distance from the controlling center, *S*, for it falls back gradually toward this center as it proceeds until by so doing it acquires velocity enough to carry it back and out to the point of contact. On the other hand, the body in the outer orbit at the point of contact has more than enough velocity to maintain its distance from the controlling center, for it gradually increases its distance until by so

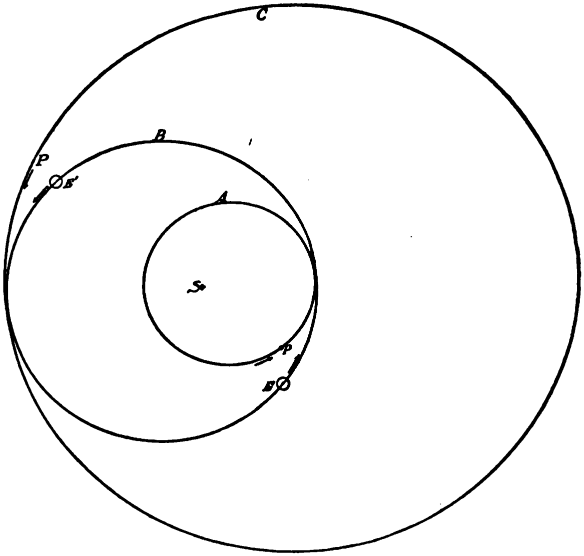

FIG. 6.—Diagram illustrating the condition under which collisions may take place in elliptical orbits of the planetary type. *S* represents the solar mass at the center of the system, *E* the planetary nucleus, *B* its orbit, *p* a planetesimal in the orbit *A*, smaller than *B*, and *P* a planetesimal in orbit *C*, larger than *B*. The case has been so chosen as to represent at once the smallest and the largest orbits of typical eccentricity that can come into contact with the orbit of the planetary nucleus. The minimum extreme is found when the aphelion point of the small ellipse *A* coincides with the perihelion point of the orbit of the planetary nucleus *B*. In no other position can the orbit *A* touch the orbit *B*. The maximum extreme is found where the aphelion point of *B* coincides with the perihelion point of *C*. In no other position can these orbits touch. Between these limiting phases, represented by the orbits *A*, and *C*, there are an indefinite number of possible planetesimal orbits that might cut the orbit *B*, but in all cases, except where the orbits were like *B*, conjunction could arise only when *a more or less* aphelion portion of an inner orbit touched or crossed a more or less aphelion portion of *B*. If the orbits were equal, the velocities at the crossings would be equal and the rotating effects would be *nil*, or neutralized, and if they were nearly equal, the difference would be slight, so that the effective cases are those of the extreme classes represented. Further explanation is given in the text.

doing its velocity is sufficiently reduced to permit it to swing back and in to the point of contact. It appears therefore that *at the point of contact* the body in the *larger* orbit is moving *faster* than the body in the *smaller* orbit, *a precise reversal of the traditional deduction that served as a prohibitive ban.*

This, however, does not cover the whole case. A simple reversal of the traditional dictum falls short of the essential truth, for elliptical orbits may come together in a multitude of ways, and from these, contrary effects may arise, though the mean effects are of the same general phase as in the special case chosen for illustration. For example, let the outer swing of a smaller elliptical orbit cut across the inner swing of a larger elliptical orbit. The effects of an encounter will then depend on the precise point at which the collisional stroke takes place, even though the body in the larger orbit is moving faster; for if this faster body in the outer orbit overtakes the slower body in the inner orbit as the latter is approaching the crossing, the inner side of the swifter body will strike the outer side of the slower body and the joint rotational effect will be forward; but if the slower body in the inner orbit has already passed the crossing, the outer side of the swifter body will strike the inner side of the slower body and the joint rotation will be retrograde. Between these two cases there is an ideal center-to-center collision with no rotatory effect. When a multitude of cases are involved, as in the growth of a planet from many small bodies, all possible phases are likely to be realized and the rotational result will be merely the algebraic sum of the diverse effects. What the balance of opposing tendencies will be can be foreseen only from the probabilities of the case; these may not be decisive in particular cases, but will be trustworthy for the majority of cases.

If the little bodies to be gathered in to form the planet are very numerous and their irregularities of distribution and of movement are such as to give much the same effect as uniformity in the net result, the spaces within which the two classes of collisions are liable to take place may be taken to represent fairly the probabilities of the case. These spaces for that belt in which the opposing effects are most pronounced and important, and essentially decisive, are shown in Fig. 7.

It will be observed that the inner section in the cylindrical track of the planet wherein collisions favor forward rotation is notably greater than the outer section wherein collisions favor retrograde rotation. If similar belts, within the two chosen, were inspected, until the whole of the planet's track were covered, the results would be of the same nature. The differences in the opposing effects would be found to decline as the belts approach one another and the rotational effects to trend toward zero. Of course the distribution of the small independent bodies may not be uniform, even in the aggregate, and the effects of their inequalities may offset, in greater or less degree, the normal advantages in favor of forward rotation. So also, their distribution may be such that the net impact values on one side or the other of the orbital plane of the planet may exceed those centered in that plane, and a more or less oblique rotation may result; indeed this is almost inevitable, and it may rise to notable consequence. In harmony with this, nearly all the rotations of planets are oblique.

These deductions are made on the assumption that the planet is not already rotating. But if rotation is already established, the effects will be modified in a very interesting way. If, for example, the planet already

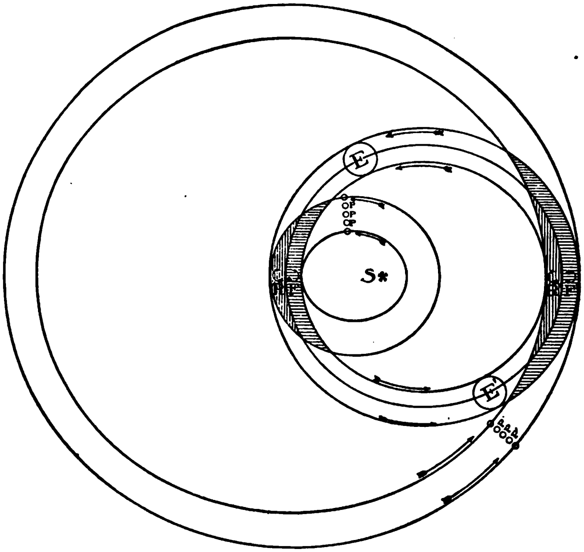

FIG. 7.—In this diagram, *S* represents the sun; *E*, a representative planet moving toward the perihelion of its orbit and about to encounter small discrete bodies, *ppp* (planetesimals), in the aphelion portion of their elliptical orbits which are smaller than the orbit of *E*, and are hence moving slower than *E*. *E* is to be imagined to be spherical and its track to be cylindrical. *E'* represents another position of the same planet moving toward the aphelion point of its orbit and about to be overtaken by small discrete bodies (*p'p'p'*) in the perihelion parts of their elliptical orbits which are larger than that of *E'* and are hence moving faster than *E'*. For simplicity, only belts of the breadth of *E* and *E'* are represented. To make a complete inspection, it is only necessary to draw similar belts between the two chosen until the whole orbit of *E-E'* is covered. On the left-hand side of the figure, the shaded area represents the only portions of the paths of *E* and of the little bodies (*ppp*) that are common and

has a forward rotation, it will be seen, on inspecting the left-hand section of the figure again, that the rotatory motion of the outer part of the planet will increase the impact values on that side—which favors retrograde rotation—while the backward motion of the inner limb of the planet will reduce the value of the impacts on that side which favor forward rotation. These two co-operating influences thus tend to counteract the normal effect which favors forward rotation. It is

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where alone collisions can take place. Since *E* is here moving faster than *ppp*, it is obvious that encounters on its inner half favor forward rotation, while encounters on its outer half favor retrograde rotation. *R* represents retrograde effect, and *F*, forward effect.

Inspection shows that the inner section of *E* within which collisions favoring forward rotation can take place is notably greater than the outer section within which collisions favoring retrograde rotation can take place, and hence, if there is an equable distribution of the small bodies, *E* is likely to acquire a forward rotation.

On the right-hand side, the small discrete bodies, *p′p′p′*, move faster than *E′* and in overtaking *E′* on its outer side tend to give it forward rotation, while if they strike on the inside, they tend to impart retrograde rotation. The preponderance of space here forces forward rotation as before. Inspection shows that the same would be true of the additional belts required to cover the whole path of *E*, *E′*, but the difference of effect would be less in these interior belts. The belts chosen are those in which the difference of effect would be greatest.

These conclusions are drawn on the assumption that *E* had no rotation at the start.

If *E* already had forward rotation, inspection of the figure shows that the rotation would tend to increase the force of the impacts in the outer section on the left of the figure favoring retrograde rotation, and to diminish those favoring forward rotation and would hence be, to this extent, unfavorable to increase of forward rotation, as explained in the main text. If *E* already had a retrograde rotation, inspection shows that the force of the impacts favoring forward rotation would be increased and that of those favoring retrograde rotation reduced. The accessions would then tend to arrest the retrograde rotation and ultimately to reverse it.

easily seen that if the previous rotation already had a certain speed, this counteracting effect would be sufficient to neutralize any assigned normal effect and there would then be no acceleration of the previous rotation. So, too, the inherited rate of rotation might be such that this counteracting effect would exceed the normal effect and tend to retard the rotation. The argument holds equally well when applied to the conditions represented in the right-hand section of the diagram where the relations of the planet and small independent bodies are reversed.

From these considerations there springs the very important conclusion that there is *an equilibrium value* for rotation in cases of this kind. If, for any reason, the planet, while undergoing accretion, acquires a rotational speed above this value, the counteracting effect of the accessions will tend to depress the rate of rotation until the equilibrium rate is reached. If the rotation falls below the equilibrium rate, the effect of the accessions is to raise it to the equilibrium value. If the distribution, or the forms, or the proportions of the little infalling bodies are changed, the equilibrium rate of rotation is likely to change also, and the net effects of further accessions will tend to change the rotation that prevailed before the change to the new equilibrium rate. There is thus an automatic regulative system by which the rate of rotation is made to oscillate about an equilibrium value.

It appears then that planetary rotations arising from the accretion of multitudes of small discrete bodies moving in elliptical orbits are more likely to be forward than backward, that they are likely to be more or less oblique, and may even be, in exceptional cases, so highly oblique as to be in effect retrograde.

It appears further that the net rotational result of many accessions is merely the equated value of their opposing effects, and that the rotation is likely to oscillate about an equilibrium value. This value is very far from the simple sum of the impact values of all the little bodies. Extremely high rotations—such, for example, as would lead to centrifugal separation—seem unlikely to arise under these conditions; more likely varying values of moderate rates only, affected by more or less obliquity, would result. An orbital state of nebular matter is, therefore, not only not a condition prohibitive of forward rotations, but is a condition distinctly tributary to it. It seems peculiarly suited to give just such rotations as our planetary family actually presents, if indeed it does not furnish the sole condition under which such a singular group of rotations could naturally arise.

CHAPTER VI

DYNAMIC ENCOUNTER BY CLOSE APPROACH

CELESTIAL KINSHIPS

The inquiry which, at the outset, had led to destructive criticism and later to futile constructive efforts on old lines, now turned into a path of its own. The field of promising endeavor had been greatly narrowed. The previous inquiry had given hints of dynamic kinships in the celestial kingdom. These, emerging later into clear light, broadly defined the issues and sharpened the criteria. The dynamic features of the solar system had betrayed two distinct genetic strains, one clearly featured in the central body, where great mass, low momentum, and an oblique attitude were generic characteristics; the other, equally clearly featured in the brood of attendants, where high momentum, low mass, and an appressed form were generic characteristics. While without doubt the planetary brood were the offspring of the central body, decentralized factors of the sun, there were unmistakable signs of another parent. The planetary system must clearly have had a bi-parental origin; it must have been dioecious, as a botanist would say. If so, all mono-parental or monoecious modes of generation must be regarded as excluded. No form of self-segregation from primitive chaos or quasi-chaos, no form of self-partition of gaseous, quasi-gaseous, meteoritic, or sporadic aggregates, no form of self-generated centrifugal separation of a common mass, could supply the two dynamic strains so distinctly portrayed in the genetic features of the solar family. Each of the monoecious modes might perhaps be the fissiperous or parthenogenic parent of appropriate celestial families born under suitable conditions, at various times and in various places in the celestial kingdom, but not the parent of our planetary family.

Partition by collision, on the other hand, is a bi-parental process and its offspring should betray their double parentage and bear bi-parental characteristics. Beyond question collision has stood in a parental relation to new celestial evolutions, but of a very radical decentralized type. The intensely dispersive, dissociative, and decentralizing nature of collisions seems to be ill-fitted for the genesis of such a planetary system as ours; collision apparently has had a much more radical disintegrating function to serve in celestial economy. Its very violent action is thought to have given rise to a radically dissociative species of the dioecious genus; our planetary system must apparently be the offspring of a kindred, but milder, less decentralizing, species of the same genus.

DYNAMIC ENCOUNTER WITHOUT COLLISION

A hint of what we now believe to be the true line of descent was caught from a mathematical study of satellites made by Roche long ago.[1] By a beautiful deductive investigation, Roche showed that if a satellite were made to approach its primary on an inrunning spiral, it would not preserve its integrity until it came into contact with the surface of the primary but, at a distance of 2.44 times the radius of the latter, the satellite would be torn asunder by the differential attraction of the primary, provided both primary and satellite were of the same homogeneous constitution, and provided cohesion and other modifying conditions were neglected. The special case has little obvious bearing on the genesis of planets, but it brought into a striking concrete form a principle of action which, when generalized and modified, seemed to shed a suggestive light on planetary origin.

In large bodies cohesion is of little moment relatively, when matched against gravitation. In bodies that are gaseous, cohesion is replaced by internal repellency. Even in solid bodies of the planetary order, the state of internal compression probably calls into play degrees of elastic resiliency that more than match cohesion. If gravitation were neutralized by counter-attraction, our planet would certainly expand vigorously in spite of cohesion. In bodies of high internal temperature, the elastic repellency rises to a potential source of powerful expansion. In the sun, there is a persistent eruptive tendency of great power. At short intervals, great bolts of sun-substance are shot forth at high velocities (see Figs. 8, 9, 10 and 11). This takes place without any obvious outside stimulus, or, if there be such stimulus, it is not declared. Beyond question if suitable strong stimulus from without were brought to bear on the sun, such as the differential attraction of a passing star, it would respond with eruptions of much greater intensity and mass.

It thus appears that from so simple a cause as the differential gravity called into action by the close approach of one massive body to another, there may arise a graded series of eruptions ranging from fractional ejections to profound disruption and dispersion, according to the closeness of approach, the relative masses of the bodies, and their internal state. The ejected parts will pursue such courses as may be imposed on them by the new forces of attraction brought into play by the changing relations of the two bodies, both of which are necessarily in swift curving motion, while one or both are losing mass by disruptive action.

FIG. 8.—Solar prominences illustrating the eruptive state of the sun. Photograph taken during eclipse May 28, 1900, Yerkes Observatory.

ASSIGNED ORIGIN OF SPIRAL NEBULAE

To follow into concrete details a typical celestial incident of this kind, let it be assumed that the general order of things in the heavens, at the time our planets were born, was not radically different from what it is today. Let the critical event be nothing more unusual than the approach of one star to another, an inevitable event, since stars move in a multitude of directions and at varying speeds. The degree of closeness of approach may obviously range from actual contacts to the utmost distance at which gravitative stimulus will be effective in promoting great eruptions. Center-to-center collision is of course the last term in closeness of approach, but that belongs to another genus. A merely glancing collision is the next door neighbor to the closest approach without collision; between these two lies the generic line of division. That collisions occur seems to be implied by the flashing forth of new stars, an event not very infrequent. According to the law of probabilities, there should be a very much larger number of approaches that escape collision—and yet are relatively near—than of collisions. Inspection of the conditions of the case seems to show that a very close approach is not necessary to call forth notable eruptive response when at least one of the bodies is already highly eruptive, as in the case of our sun. This being true, statistical treatment makes the number of effective approaches a very high multiple of the probable number of collisions. Even if effective results were confined to the Roche limit—which seems to be far from the case—there should be six or eight effective approaches to every collision. In such close approach as this, however, complete disruption of one or both the bodies would apparently take place, and that is far too violent to fit the case in hand. While it is possible that quite a small star, or other small body, closely approaching the sun might develop a nebula fitted to form our planetary system, only a much more distant approach of an average star would be suited to call forth such relatively small ejections as those involved in the genesis of our planets.

A close approach involves a very substantial encounter, though it is a purely dynamic encounter. It is not difficult to visualize it, if the spheres of attractive force are pictured as surrounding the approaching bodies and as plunging into one another. The encounter is very real, however intangible. The action may be pictured as a conflict of opposing attractions; or as an overplacing or interpenetration of attractions; or as a neutralization of attractions. In either case, the master body disrupts the minor body, or tends to disrupt it. Each body stimulates any internal tendency to eruption that may affect the other body. The matter that may be shot forth will be drawn into some one of several possible courses by the joint attractions of the two bodies whose positions and distances are constantly shifting.

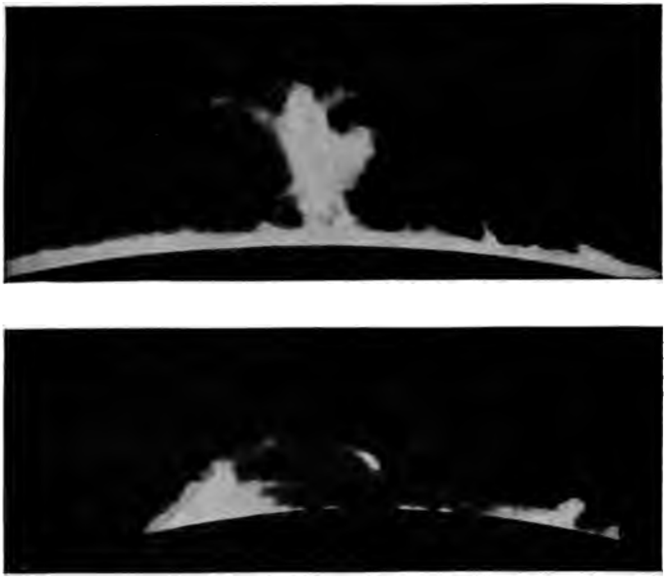

FIG. 9.—Eruptive prominences of the sun. Yerkes Observatory

For an illustrative case, selected to suit our problem, let our sun, in its ancestral state, be the body approached. For its partner in action, let a more massive star be chosen and, for convenience, let it be so dense and inert that its response to the reaction of the sun upon it may be neglected. In addition, it will be convenient to speak of the relative changes of position of the two as if the whole motion were made by the passing star, though in the accompanying illustration (Fig. 17), both bodies are represented as moving about their common center of gravity as they actually do.

In selecting the closeness of approach, let us observe that only 1/745 of the sun's substance was required to form our whole planetary system. There are now known to be eight planets, twenty-six satellites, and about eight hundred planetoids; probably the whole number of the latter may ultimately be found to be a thousand or so. The average mass of these solar attendants is thus only about 1/745,000 of the mass of the sun. The average mass of the planets, neglecting the planetoids and satellites, is about 1/6,000. Even the largest planetary mass is less than a thousandth of the mass of the sun. It was not necessary, therefore, that the sun should give forth even so much as one-tenth of 1 per cent of its substance to form the largest planet, assuming that the whole material for the planet was ejected from the sun by a single impulse. The requirement for the earth would be about one three-thousandth of 1 per cent of the sun. It thus appears that the draft on the sun to supply the substance of the planets was very small relatively. This suggests that the passing star, if it had the mass we have chosen, must surely have kept well away from the sun to have had such slight stimulating effect as the case required. We assume therefore only a quite distant approach.

Whether the sun was more strongly eruptive at the time the planets were born than it is now, or less strongly eruptive, is a matter upon which perhaps two opinions may be held. It does not seem material to us here to know which alternative was true, since the appropriate intensity of disruptive effects could be easily attained by a nearer, or by a more distant, approach of the passing star. Let it be assumed, however, that the eruptivity of the sun was of the same order then as now.

At present, the sun is almost daily shooting forth gas-bolts of vast dimensions and often at such velocities that they rise many thousands of kilometers above its glowing surface (see Figs. 8, 9, 10, and 11). Conservative computations assign these eruptive ejections velocities occasionally reaching one hundred or two hundred kilometers per second, though the average speed is less. Estimates by observers of high standing assign much higher velocities in certain cases, some of these rising to several hundred kilometers per second; indeed, velocities that surpass the sun's power of control have been announced. But, in an inquiry that seeks to keep well within the bounds of probability, it is not wise to press the case to its limits, and there is no need for this, since extremely high velocities independent of stimulus are not critical to the issue. Even if it were necessary to suppose that the eruptions were confined to velocities much lower than the most conservative interpretation of the observations would permit, it would merely require us to suppose that the passing star

FIG. 10.—Eruptive prominence of the sun photographed at Yerkes Observatory March 25, 1910, at 4h 14.7m.

FIG. 11.—Same prominence as above photographed March 25, 1910, at 4h, 57.9m, or 43.2m, later.

came somewhat nearer the sun in evoking the ejections the case requires. Even if the eruptive prominences of the sun were essentially illusions, eruptive action of adequate effectiveness would arise from a sufficiently near approach without making serious drafts on the full disruptive potency attainable by approach. And so, without bias from any necessity of the case, we are free to choose for our working model such measure of the prodigious eruptive energies now resident in the sun as seems safest or most probable.

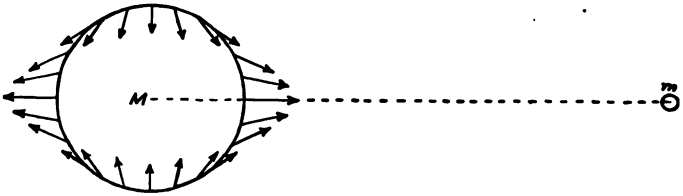

FIG. 12.—Diagram of tidal forces showing lifting forces in due proportions and directions to and from the moon and the girdle of compressive forces at right angles to these. Note that the direct compression is half that of direct lifting. Prepared by F. R. Moulton.

To this group of inherent properties, energies, and activities, let there now be added the gravitative potencies of a great star passing by. Its attraction on the several parts of the sun necessarily differed because of differences of distance. The effects of the star's attraction were of the type made familiar by the study of the tides. Its differential attraction must obviously have reduced the gravitative pressure in the interior of the sun along the line that joined the centers of the star and the sun; in other words, there must have been a tidal elongation. Let it be recalled that such a tidal response takes the form of "bulges" on opposite sides, one toward the attracting body, and one on the opposite side (see Fig. 12). Between these bulges, and at right angles to their axis, there is a girdle of tidal compression arising from a component of the oblique attraction exerted on these parts by the tide-raising body.

If the lifted portions that form the "bulges" were plotted as though lifted from a plane, instead of a spheroid, they would appear as cones (see Fig. 13). Such conical forms would represent truly the way in which the lifting force of the tidal pull is distributed over the tide-lifted area. The bulging form, as we style it, arises from the curved base on which the lifted portion is superposed. There is a certain merit, therefore, in the use of the term tidal cones, rather than tidal bulges.

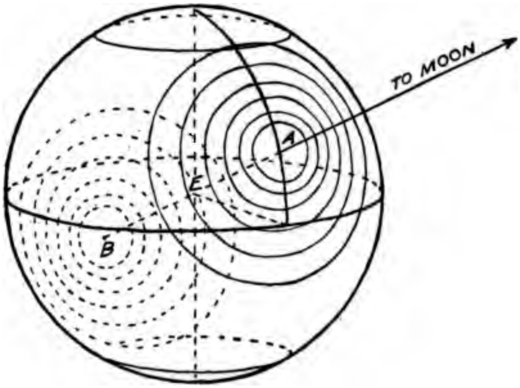

FIG. 13.—Diagram showing the tidal cones, *A* and *B*, pointing to and from the moon. Prepared by F. R. Moulton.

Picture the passing star, therefore, as having eased, by its differential attraction, the pressure internal of the sun along the axis of the tidal cones, while it has added to the pressure at right angles to them. Under the law of least resistance, it is clear that this would have predisposed the eruptive forces within the sun to ease themselves along the lines of this reduced pressure, rather than in directions of higher pressures. Any previous tendency to eruption at right angles to the axes of the cones would necessarily feel the restraint of the increased pressure in those directions. There would therefore be a concentration and intensification of eruptive action in the axes of the cones and a restraint and reduction elsewhere. Even now, owing to causes not yet determined, the eruptive action in the sun is concentrated in certain sub-equatorial belts. It is also subject to periodic fluctuations. Both of these indicate its susceptibility to concentration. There is, therefore, firm ground for the inference that the eruptive action, under the conditions sketched, would have been centered in the tidal cones, and would have been much more massive and more forceful than under normal conditions.

In accordance with well-known tidal principles, one set of the eruptive projections must have been shot directly toward the passing star and the other set in the opposite direction. The behavior of the latter is not readily visualized, unless one has long accustomed himself to see just how each part of the body is moving and what is the balance of value between its inertia and the sum of the attractions acting upon it. It is all, however, a matter of perfectly consistent differential action under the existent states of motion, however paradoxical it may seem. The realities of the case are beyond question. For convenience, therefore, let us follow merely the outshoot toward the passing star, and add, constructively, in accordance with demonstrated mechanics, the distal outshoot. The outshoot toward the passing star should be a little greater than the outshoot on the farther side of the sun.

The eruptive prominences of the sun are quite various in form and dimensions—see accompanying figures—but it is permissible to idealize them as bolts of sun-substance, though the bolts must be pictured as protean in form and as accompanied by much scattered material. A succession of such bolts, attended by fragments, streamers, and diversely scattered matter, naturally takes on the semblance of a chain of eruptive projectiles.

The initial course of the gas-bolt, as it left the surface of the sun, would be directly toward the passing star. If the attraction of the star be for a moment neglected, the bolt, after going out as far as its impulse could carry it, would fall back to the sun, provided of course that it was not shot entirely beyond the sun's control. But when account is taken of the attraction of the passing star, the course of the ejected mass is vitally dependent on the precise balance of attractions and inertia; for, while the bolt was moving out and falling back, it would have been drawn aside in the direction of movement of the passing star, since the pull of the star was always shifting to a new line directed to its new position. A tangential element would thus be introduced. Now the relative amount of this forward or tangential pull is a critical factor; its value is obviously dependent on the relative distance to which the bolt was projected. A multitude of cases may arise, but they may be grouped as follows:

1. If the gas-bolt were not projected far out, relatively, it would fall back to the sun without being very much drawn forward by the passing star. It would, however, in falling back to the sun, carry such transverse momentum as it had gained by the forward motion imparted by the pull of the star. This increment of transverse momentum would affect the rotation of the sun, adding to it, if the sun were rotating in that direction; reducing it, if the sun were rotating in the opposite direction; or modifying it partitively, if the sun's rotation were in an oblique or transverse direction. As the sun's rotation is now singularly slow, and as its axis is appreciably oblique, it is suggested that the sun's rotation may originally have been essentially opposite to its present rotation, and that a series of sun-bolts, shot out short distances and drawn forward by the passing star, in falling back carried enough tangential momentum first gradually to arrest the original rotation of the sun and then gradually to impart a slow rotation in the direction pursued by the passing star. This then may be regarded as the solar rotational group of short-distance projectiles.

2. If the gas-bolt were shot a certain larger proportion of the distance to the passing star, the latter would draw it so far forward that, on returning toward the sun, it would fail to strike the solar disk; sweeping by, it would fall into an elliptical orbit about the sun. If the bolt were shot a still larger part of the distance to the passing star, the forward pull would be relatively greater, and, in falling back, the bolt would give the

sun a wider berth and fall into a more open orbit. And so, by different degrees of projection, there would naturally arise a series of orbits of varying degrees of eccentricity and of attitude. This may be regarded as the planetary group of projectiles, for out of these our scheme assumes that the planets were aggregated.

3. If the bolt were shot out to a certain still larger part of the distance to the passing star, the pull of the sun upon the bolt would be so balanced that the course of the bolt would be veered into a tangent between them and elude the control of either. This then falls into the group of escaping projectiles; these pass beyond the province of our special problem.

4. If the bolt were shot beyond this critical distance, it would pass into the sphere of control of the visiting star, and would probably become a secondary to it. In a possible case, the bolt might actually plunge into the visiting star, but this would be highly improbable if the star were passing at considerable distance as we have supposed. This then represents a group of projectiles that transfer allegiance; they illustrate how stars may gain or lose.

These are all cases that arise from distant approaches of a type supposedly fitted to our problem, and we do not need, in this connection, to consider other cases, however vital they may be to a complete theory of spiral nebulae. But to avoid occasion for misapprehension because of too complete neglect, let us note, before passing on, not only that much more vigorous eruptive and disruptive action would necessarily spring from the closer class of approaches of stars to one another, but that the positions of the two stars relative to the gas-bolts might, in some of these cases, be rather radically different from those just considered and that with such various changes the effects would be correspondingly different. In the first two of the four cases just considered, the bolts are made to turn backward before they get halfway to the star that evoked them. They do not pass outside the sun's sphere of control even temporarily. But, on the other hand, if we consider the cases in which the stars make very close approaches, it is necessary to note, in the first place, that there is little room between the stars for deployment of the kinds above sketched and, in the second place, that the eruptions are likely to be so violent as to project most of the outshoots quite beyond this limited space and introduce a new set of cases of extreme interest whose tracing is attended by extreme difficulty. A tentative suggestion of their general nature is all that will here be hazarded:

a) If the passing star curved close about the sun, the eruptions would necessarily be violent and, being shot directly toward the passing star, they probably might, in certain cases, actually impinge upon it. In such a case the nebulous train on the side of the passing star would actually connect it with the eruptive body, as seems to be suggested by M 51 Canum Venaticorum (Fig. 14).

b) In most cases, however, it seems probable that the very high speed of the passing star in curving about the sun would carry it out of the projectile's path before actual impact could take place. Though the projectile would no doubt be much deflected toward the star, it might none the less fall behind it. Probably actual collision of the projectile with the evoking star would only be imminent when either the approach was extremely close or the eruptivity of the projecting star was very intense. If the passing star escaped the shot it evoked, as seems probable in most cases, and the projectile, crossing the star's path behind it, shot onward, both star and sun would thereafter *unite* in restraining and deflecting its course instead of contesting one another's influence as in the cases just previously considered. Very declared results, marked by great diver-

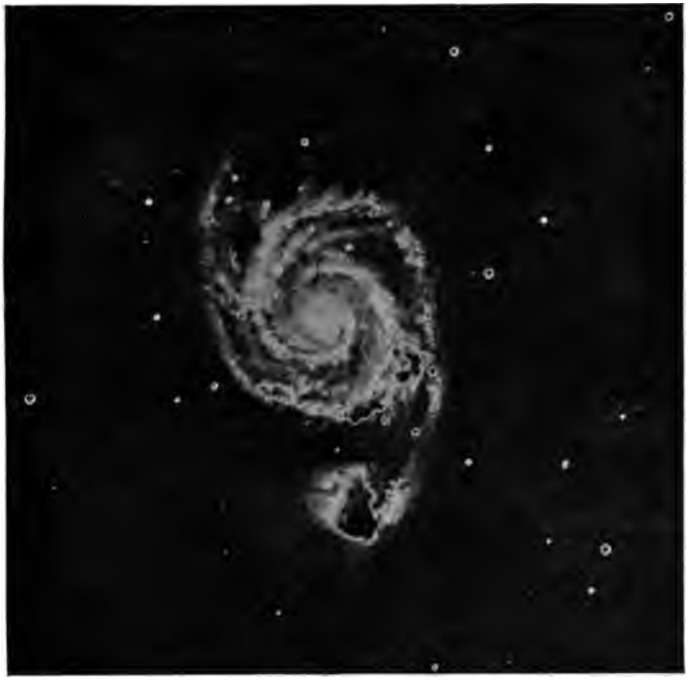

FIG. 14.—The remarkable spiral nebula, M 51, in the constellation Canum Venaticorum. Photographed at the Yerkes Observatory.

gencies of paths and differences of velocity, might well be expected.

With one possible exception, all this group lie outside the bounds of our present problem. It is possible, perhaps not improbable, that a relatively small body passing near the ancestral sun evoked eruptions of an order suited to the formation of a planetary system of the type of our own. This possible case throws the chief burden of action on the eruptive potency of the sun and takes the case essentially away from the simple Roche principle, for in this case a quite inferior body supplies the stimulus for the eruptive projection. The elements of the case are less within the range of celestial observations and the dynamics are extremely difficult. The formidable task of tracing out such a case has not been attempted.

In general, however, the projectiles in cases of such very close approach might be expected to be shot forth very violently, to suffer great deflections in passing close in the rear of the evoking star, and to pursue highly varied courses and attain wide deployments. Nebulae, so produced, are plausibly assigned to the giant class not so much because of their massiveness as because of their wide deployment. When really massive, they are the possible parents of star clusters or at least of decentralizations of a higher order than a planetary system of the solar type.

Let us return to the simpler case whose elements we have selected with a view to planetary genesis by distant stellar approach. In such simpler cases the logic of the assigned movements seemed altogether firm and the conclusions inevitable. But to make these deductions doubly sure, and at the same time to test, in some measure, the quantitative value of the deploying process, and to determine the nature of the orbits that would be developed, Dr. F. R. Moulton, with the computative aid of Dr. E. J. Moulton, followed out mathematically the courses of representative units, projected in this way, in nearly half a hundred hypothetical cases. The process was found to be unexpectedly effective; the orbits took on a wide range of dimensions as well as much variety

FIG. 15.—Spiral nebula HV 2 Virginis. Photographed at the Lick Observatory.

of configuration. All the four types of results sketched in the first list above were encountered in the first ten cases investigated. In his computative inquiry, Dr. Moulton traced out the orbits of the projectiles shot from the side of the sun opposite the passing star, as well as those shot toward it. The paths of the two sets of projectiles were found to be curved in the same sense.

For obvious reasons, the planes of all such orbits must lie in or near the plane of movement of the passing star. The whole group of orbits must thus take on a discoidal configuration such as characterizes our planetary system and the whole class of nebulae assigned to this mode of origin (Fig. 16).

Stars do not pass near one another in straight paths. Under the law of the heavens, they pay obeisance to one another by deviations from their previous courses. These usually describe hyperbolic curves. At long distances, the stars follow their normal courses with but slight deviation; with closer approach, there is increasing curvature toward one another; when they are nearest one another, there is a sharper and swifter turn. If the approach is very close, their speeds at the climax become extremely swift, and the turn of the stars about one another correspondingly sharp. During the stages of closest approach, the positions of the stars relative to one another are rapidly changing, and hence the tidal cones are constantly shifting their positions in the sun, as well as their directions in space. From this there arises inevitably a series of bolts shot out in succession which take new directions at each successive instant. As a mechanical necessity, the chain of such successive bolts takes the form of a spiral. This may be seen in detail by following out the courses of the projectiles represented in the accompanying diagram (Fig. 17). A case of rather distant approach and mild action, suited to the development of a small spiral nebula, was chosen for this illustration. It will be noticed that the paths pursued by the projectiles are not identical with the spiral chain of projectiles into which they are forced to arrange themselves. The divergence

FIG. 16.—Edge view of spiral nebula HV 24 Comae Berenices, showing its highly discoidal form. The dark band is probably due to light-absorbing nebular matter. Photographed at the Lick Observatory.

between the paths and the chain of nebulous matter may vary widely. A much closer approach to coincidence between the paths of the projectiles and the chain of projectiles is assignable in certain cases of closer approach and more violent projection.

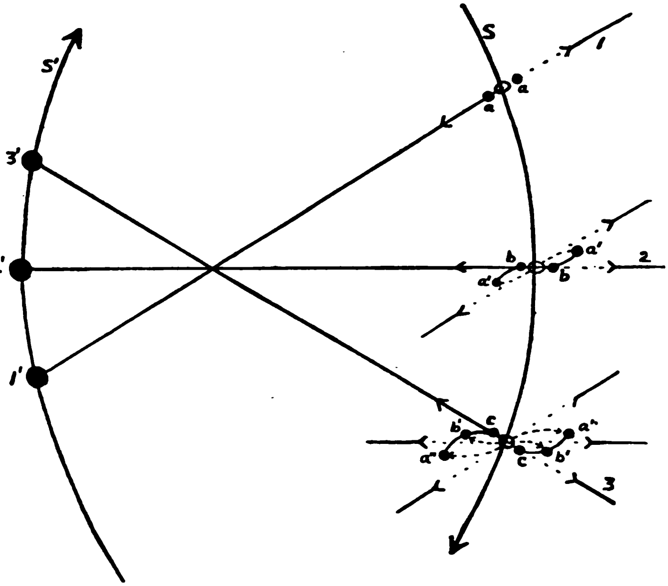

Fig. 17.—Diagram showing one of the assigned modes of development of a spiral nebula.

By such an analysis of the consequences of the close approach of a massive body to an eruptive star, we are led to a very definite concept of the way in which a spiral nebula may be developed. The causal event is one of the simplest and most inevitable. When the multitude and variety of the bodies subject to close approaches are considered, as well as the innumerable degrees of possible closeness of approach, it is clear that the range of effects may be extremely wide, and that the maximum deployments may be very impressive. On the other

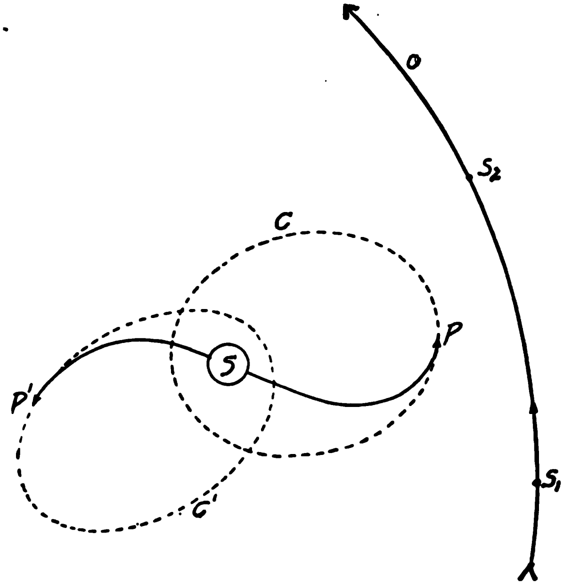

FIG. 18.—Diagram showing an assigned mode of developing orbits (Moulton).

hand, the milder effects must grade away to the unrecognizable. A great series of spiral nebulae ranging from very wide deployment down to the indistinguishable seems to be an almost necessary inference from the nature of the process and the variety of the conditions. The nebulae should, however, all have a common fundamental character. The multitude of spiral nebulae known to exist is in close accord with this deduction. The same is true of their variations in magnitude and configuration. If this is the true interpretation of their origin, it is not at all strange that they should outnumber all other classes.

THE TESTIMONY OF THE NEBULAE

Among the distinctive features of the spiral nebulae are the two arms, or groups of arms, that spring from diametrically opposite sides of the nucleus and wind outward with similar curves, but rarely in precisely the same way, and almost never to the same length. This singular feature is admirably explained by the assigned tidal genesis. The explanation holds good even to such details as the persistent inequality of the arms, and to their differences in configuration.

It is to be noted that the arms must be supposed to begin to change their curvature more or less as soon as they are projected. In so far as a nebula is under the effective control of its center of gravity, the inner parts must move faster than the outer ones. If the residue of the central mass remains large after the arms are thrown out, and these are not projected far, speaking in astronomic terms, the arms must, after the projectile motion is brought under control, wrap up so closely in a short astronomic period as to be almost or quite indistinguishable, at stellar distances, from a continuous disk. A knotted aspect might still be presented; disks with knots so interpretable are observed. Small spiral nebulae probably thus come to closely simulate planetary nebulae in form, but probably the spectra remain distinctive. If, on the other hand, the deployment of the spiral is great and the central part has been largely consumed in this deployment, the central gravitative force is likely to be feeble, and its influence on the revolutionary motion relatively unimportant; the wrapping, if it obtains, may be extremely deliberate. If the projection is of a still higher order, the deploying movement may continue for a long period.

FIG. 18*a*.—A spiral nebula in which the two arms are especially distinct, HI 55 Pegasi.

If the outshoots from the parent star are so vigorous as to pass beyond the sphere of control of the parent star, or what is left of it after their expulsion, the projected matter will normally not assume closed orbits about the center of the nebula but will continue in courses divergent from it. This may be one of the modes of sowing the germs of star clusters. Such uncontrollable deployments probably only arise from the very close approach of very massive bodies endowed with a very high explosive competency. As repeatedly noted, the genesis of our planets is a problem of a much more modest order.

In harmony with these variations of mass relations, velocities, and other influential conditions, the spiral nebulae present a great variety of special forms, while they retain, with much fidelity, the basal features that imply a decentralizing process in which a large tangential or rotational element is indicated.

Most of the peculiar features of the projected matter, its knots, blotches, trails, and haze, seem to find an adequate basis of elucidation as the natural belch-products of the solar eruptions under high external stimulus. An inspection of the accompanying photographic illustrations (Figs. 15, 18*a*, 19, 20, 25), with the suggested genesis in mind, is invited. The illustrations are chosen almost necessarily from the larger varieties of spiral nebulae, though these do not well fit the case in hand. Small nebulae, with only scant and short projections, such as are suited to be the parents of little planetary systems like ours, are, with little doubt, nearly or quite beyond the reach of the telescope at average stellar distances, and besides, they probably wrap up to scarcely distinguishable forms very soon after formation.

The greatness of the deployment of the giant spirals has sometimes been thought to take them out of the category of objects adapted to planetary genesis. Quite likely their functions go far beyond planetary genesis and include the rejuvenation of stars and the formation of star clusters. But it may be remarked that, so far as dynamical requirements are concerned, their great deployment means much less than it gets credit for, since all but a small part of the force required for the great projection is consumed in the early stages in the vicinity of the parent star where the opposing gravitation is effective. After the deployment has reached the dimensions of a small nebula, only a very small additional percentage of the total force is required to push the deployment to any degree of dispersion, even indefinite dispersion.

The amount of matter involved in the giant nebulae is purely a matter of interpretation. Nothing observed in the heavens necessarily implies great mass. On the other hand, the known masses of stars suggest great possibilities of spectacular impressiveness, if properly deployed. They also suggest large possibilities of producing germ-suns ready to begin new careers of growth. Jupiter, though but a thousandth part of an average star, barely escaped being a sun. Judiciously divided, our sun might probably be the parent of a hundred germ-suns ready for careers of growth. The giant suns might give rise to some thousands or possibly tens of thousands of embryo suns. There seems no cogent reason, then, in any known consideration, why a star of the larger order, vigorously decentralized by very close approach to another massive body, may not furnish all the matter in any of the giant nebulae, however spectacularly displayed. The deep impressions formerly made by the even more spectacular apparitions of comets should perhaps offer a wholesome suggestion of

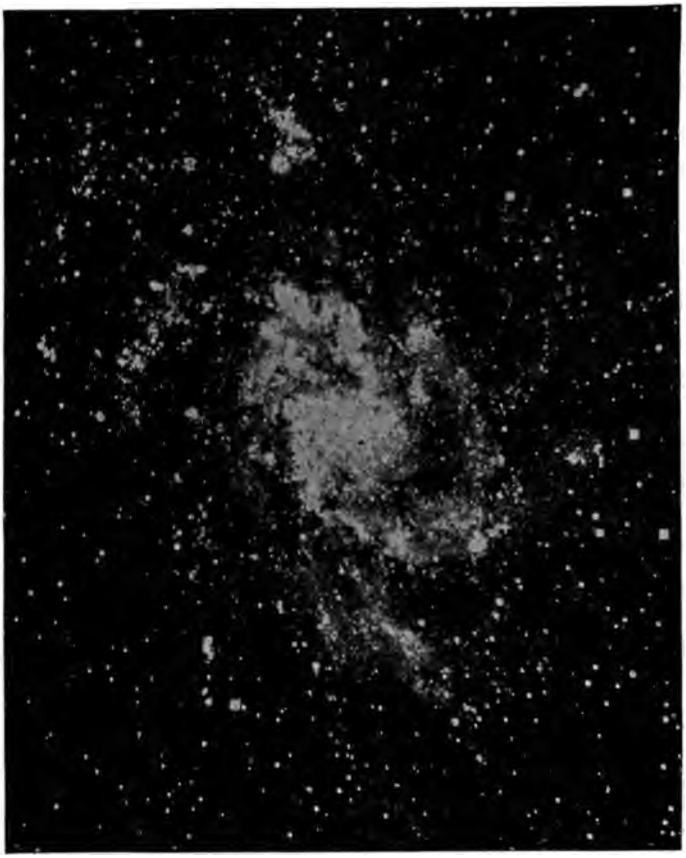

FIG. 19.—A strongly deployed giant nebula, M 33, in Triangulum. Photographed at the Yerkes Observatory.

restraint in estimating the mass-values of the giant spiral nebulae.

Looked at from the dynamical point of view, it appears that a little spiral nebula, generated in the way outlined, should have the germs of those extremely exacting dynamical qualities which our planetary system embodies. The suggestion behind this is that dynamic encounter is one of the common and effective modes of stellar rejuvenation.

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CHAPTER VII

THE EVOLUTION OF THE SOLAR NEBULA INTO THE PLANETARY SYSTEM

The term solar nebula is not here used in the inherited sense of a nebula that condensed *into* the sun and its attendants, but as a nebula evoked *from* the sun to form its attendants. As here interpreted, the solar nebula was little more than a streaming knotty pair of arms of nebulous matter shot out from the sun and curved into spiral appendages about it by the joint pull of itself and a passing star.

TEST BY THE LAW OF PROBABILITIES

To give the precise results actually realized in the solar system, even so simple a genesis as that here assigned must have involved the contributions of four elements, each of which might, theoretically, have been very different from what it actually was, and hence a different system might have arisen: (*1*) *the star's path*, which might have lain in any direction; (*2*) *the movement of the star in this path*, which might have been either to or fro; (*3*) *the plane of the sun's rotation*, which might have had various attitudes, and (*4*) *the rotation of the sun in this plane*, which might have been either to or fro. It would be very remarkable if the path of the star and the direction of its motion should have concurred with the plane of the sun's equator and the direction of the sun's rotation. This, indeed, was one of the multitude of possibilities of the case, but the probabilities were overwhelmingly against it. The law of probabilities would lead one to expect such unconformities and discordances as spring from random combinations. This seems to afford a severe test, for our postulates can only be those which the dynamic vestiges of the system dictate and the logic of the case requires, and these are rather exacting. They seem to require (1) that the path of

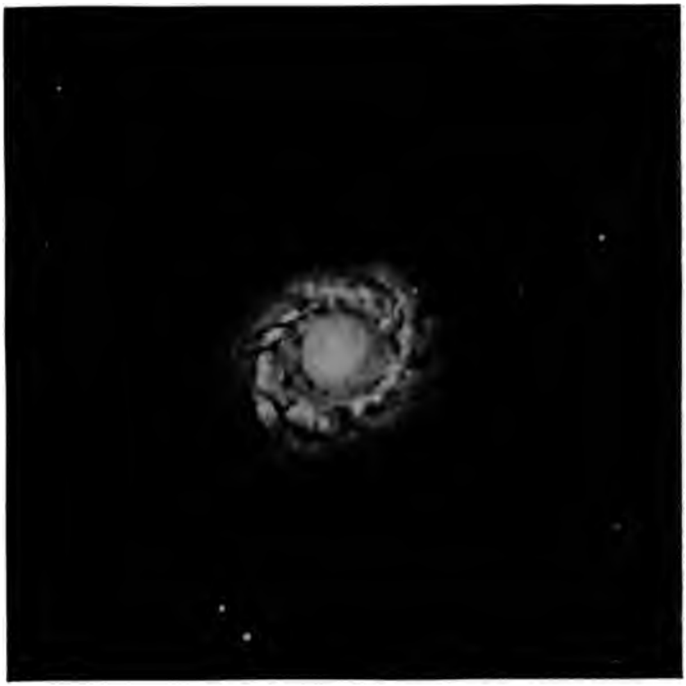

FIG. 20.—A spiral nebula with large knots wrapped about a large center. M 94 (N.G.C. 4736) Canum Venaticorum. Photographed at the Mount Wilson Solar Observatory.

the star should have lain nearly, but not quite, in the invariable plane of the planetary system; (2) that the movement of the star in its path should have had the direction in which the planets now revolve; (3) that the equatorial plane of the sun should have differed from its present plane to the extent of the effects of a certain component of the value of the momentum carried back to the sun by the returning projectiles after they had been drawn forward by the passing star, as explained in the last chapter; and (4) that the sun should then have rotated in a direction as nearly opposite to its present rotation as this change in its plane permitted, the reversal of the rotation being due to the remaining component of momentum carried back by the returning projectiles. If all these are restored in imagination, it will be seen that their unconformities are quite sufficient to meet the demands of a typical case of random combination. Indeed, they seem to imply about as wild a cast of the die as the law of chance could well require. *The present degree of concurrence is a forced result imposed by the mutual reactions of the agencies that entered into the combination.*

SPECIFIC FEATURES OF THE PLANETARY KNOTS

The knots of the solar nebula play a leading part in our interpretation of the immediate genesis of the planets, planetoids, and satellites, since they served as collecting centers for them. They were not only physical collectors but dynamic collectors; they not only arrested the flying matter that fell into them, but, within their spheres of control, they drew flying matter toward the collecting centers and made it more liable to arrest. There was essential need for both these functions. As a rule, minute scattered particles in the open space of the heavens move faster than the more massive bodies. They do not "float in space," but are drawn or driven hither or yon at more than average velocities. Where space is so dominated by the attractions of massive stars and combinations of stars, where the attractions of minute flying bodies are so trivial, and where the inertia of such flying particles is so great relative to their own attractions, the self-aggregation of such bodies into planets is highly improbable. The knots of the nebula, however, furnished the requisite collecting centers.

It would no doubt be the part of prudence to rest interpretation with this simple observation that adequate centers of aggregation for our planets, planetoids, and satellites were offered by the knots of the parent nebula. However, the working value of any hypothesis depends much on the elaboration of its details, so that many points of contact with the concrete phenomena it is to explain may be presented. This must be our excuse for offering some speculations of rather uncertain value relative to specific features of the knots of the parent nebula. The ground for this is far from firm and the suggestions should be held lightly, and yet they may be suggestive.

It is assumed that, at the time the nebula was formed, the greater eruptions of the sun were concentrated, as now, in two belts not far from the sun's equator. It is inferred that, as the star approached from a distance, its first feeble stimulus led only to moderate ejectments of sun-substance and that these suffered so slight deviations by reason of the forward pull of the star that they did not escape striking the sun's disk on their return and so carried into the sun a little momentum acquired from the star. This momentum neutralized an equivalent amount of the momentum of the sun's rotation, then opposite to its present rotation. With nearer approach of the star, the eruptions increased in mass and vigor with increased effect on the sun's rotation. With still nearer approach, a portion of the projectiles failed to strike the sun's disk on returning and swung into orbits about it. Later, a still larger part of the increasingly vigorous projectiles passed into orbits, and these orbits grew broader, but certain portions of the projectiles continued to return to the sun and affect its rotation.

During all this time the pull of the star was oblique to the normal ascensive lines of the sun's greater eruptions, and the sun and star worked at cross-purposes; but, as the star curved into the critical part of its path, where it made its closest approach, it passed directly over the belt of the sun's most effective eruptions, and not only the most favorable co-operation of sun and star were realized but nearly the maximum mutual attraction. It is assumed that the greatest eruptive bolts were then shot forth, and that they were projected with the greatest velocity. It is taken for granted that the stimulus of vigorous action on the side toward the star would react as stimulus to eruption on the other side, and that nearly simultaneous bolts would issue from the proximate and from the distal side of the sun. It is supposed that the action would be most effective when the first eruptive belt was crossed, for then the projectile forces drew on the fullest stores of eruptive potency in the sun. The second pair of great eruptions are assigned to the stage when the second belt of solar eruptions, on the farther side of the solar equator, was crossed. These two pairs of eruptive projectiles of the first order are assumed to have been the parents of the four great planets, the two outermost—with the peculiarities of the first-born—growing later into Neptune and Uranus; the two following, favored by the pulsations set up by the previous great eruptions and by greater facilities for growth, but lacking the fullness of eruptive resources that favored the first pair, constituted the knots that grew into Saturn and Jupiter.

As the star passed on in its perihelion curve over higher latitudes of the sun, obliquity of action recurred, and either a multitude of imperfectly associated eruptions, or a great eruption much rifted by divergent projection, gave birth to many little nebulous bunches which later grew into the planetoids.

On the backward swing, following the star's perihelion turn, the passage over the nearest zone of solar eruption was again attended by a pair of projections, but these are supposed to have suffered greatly from the measurable exhaustion of the eruptive potency of the sun involved in the expulsion of the previous great bolts and so attained only moderate masses and velocity of projection. On passing the other eruptive belt the second time, an additional pair of bolts was shot out, somewhat smaller still and less distantly projected. With this the larger order of eruptions ceased, and the less effective class of activities, such as had attended the early approach, was repeated in reverse order but, it is assumed, with lessened vigor from progressive exhaustion. This is a very special explanation and obviously does not apply to spiral nebulae in general because of the numbers and irregularities of their knots. If additional planets in our system should be discovered, the explanation would not even hold for it.

INTIMATE NATURE OF THE KNOTS AND NEBULOUS HAZE

When the immense belches of sun-substance were about to be lifted from their places deep in the sun, they must have been gaseous or potentially gaseous, and they must have contained all the chemical substances that were present in the horizons of the sun from which they came. Nearly all the known chemical elements were no doubt intimately intermingled there, after the normal manner of gases. But when these belches emerged from the sun and were shot into the approximate vacuum of surrounding space, they must have undergone great expansion, and there must have been a great reduction of their temperature as a consequence. As they traversed the Roche limit of the sun, they felt a modified form of the Roche effect which intensified the deploying influences. When, after such expansion and cooling, the nebulous masses swept out into interstellar space and entered upon their orbital courses, their temperatures suffered still further from the rapid radiation inevitable in such diffuse bodies. The vital question arises, therefore: How long was the original gaseous condition retained? Or to what degree was it retained? If it was soon lost, in whole or in part, what dynamic condition took its place? The answer is to be sought in the vestiges that remain for our enlightenment, if it is possible to discern the light that is in them.

The great planets Jupiter and Saturn have low specific gravities and probably embrace high proportions of the more volatile elements. Their great masses give them competency to hold these in spite of high molecular velocities. It is a rational inference that the gas-belches from which they sprang were also relatively massive and gravitatively competent. This is in keeping with the effective concurrences to which their birth is assigned. It is assumed, therefore, that at all times in the history of the great gas-belches that gave rise to Jupiter, Saturn, Uranus, and Neptune, their self-gravity was competent to control not only the average planetary molecules, but the lightest and most active molecules shot out with them from the sun. These great knots, therefore, probably had at least gaseous centers, even when most expanded and cooled, and were perhaps largely gaseous bodies at all times. They probably retained rather high temperatures. By reason of their great attractive powers, they probably gathered in effectively stray molecules and random aggregates which the smaller knots could neither assemble nor hold; hence not only their present preponderant masses, but their relatively low specific gravities.

In contrast to the intimations of these great bodies of low mean specific gravity, are the suggestions of the planetoids and satellites which now hold little or no gaseous envelopes and whose mean specific gravities are relatively high. The terrestrial planets are more nearly akin to these than to the great planets, so far as the gaseous element is concerned. In their present full-grown state, the earth and Venus have fair atmospheres; Mars, a rather scant atmosphere; Mercury, little or no atmosphere. The knots from which they grew should have been much less competent to hold the active gases, while the high temperatures that attended their emergence from the sun, by increasing the molecular activities of the gases, further reduced their competency.

Perhaps we need to turn aside a moment here to inquire: How much less massive were the knots than the full-grown bodies? A considerable series of comparisons between the apparent amount of matter in the knots of various spiral nebulae and the apparent amount of matter not so bunched indicates that the knots may perhaps contain a quarter, a third, or a half of the whole. Considerations connected with the control of the moon-knot by the earth-knot from the outset seem best satisfied by assigning the earth-knot 30 or 40 per cent of the earth's adult mass. Neither of these estimates will bear much emphasis, for the basis in both cases is infirm; the light of the nebulae, by which their masses were judged, may not be a safe guide, and the assumptions of the lunar deduction are not imperative. The estimates merely serve as a rough working basis. Duly discounted they yet make it clear that after the smaller gas-bolts had been shot from their compressed state in the sun into the approximate vacuum of interstellar space, their competency to hold the more active molecules was distinctly limited, even in the case of the terrestrial planets. So far as concerns the little knots that were to grow into the planetoids and satellites—and into Mercury and Mars as well—it may be concluded with confidence that such gases as now form our atmosphere could not have been held under control at the high temperatures at which they were shot from the sun. The light active molecules that mingled in these smaller gas-bolts must probably, as they issued into open space, have been scattered hither and yon by the force of their own collisional reactions, and must have entered on paths of their own under the direct control of the sun, but free from the control of the bolts with which they were shot forth. If so, they no longer constituted bodies of gas in the true kinetic sense. In following orbital paths about the sun, they had the dynamic qualities of little planets, that is, they were planetesimals. This name has been introduced to designate all such small bodies—whether atoms, molecules, or aggregates—as behave like minute planets. This is done to distinguish them from the constituents of gases, from most meteorites, and from other minute bodies that follow heterogeneous paths and that have, as a result, different dynamic qualities.

Contrasted with the escape of very light active molecules from the smaller knots, was the tendency of the heavy sluggish molecules, and of such aggregates as may have been formed, to remain assembled under their own mutual gravity. The mean specific gravity of the moon is 3.4; that of the earth 5.5; that of Mars, Mercury, the planetoids, and the satellites generally, so far as determined, of the same general order. A part of this high specific gravity is due of course to compression, but aside from this their inherent specific gravity is high. It is easy to be misled as to the average specific gravity of earth-substance. The atmosphere and the hydrosphere make a brave show at the surface—whence we are compelled to view the earth—but they really constitute only a trivial fraction of earth-substance. The great mass of the earth is made up of much heavier substances, and it is the behavior of these heavy substances that constitutes the essence of our present problem. Few of these substances could remain volatile except at very high temperatures. Hence on emerging from the sun and undergoing the great expansions and the effective radiations incident to this, a large portion of the more refractory material in the smaller order of knots probably fell to the liquid or solid state. In the discrete state that ensued, combined with the influence of their projectile impetus, the assemblage of particles could scarcely collapse as a unit; the particles could probably only coalesce, with such other particles as crossed their paths, into minute scattered aggregates, or else remain as independent molecules. In so far as any matter of this heavier order remained in a free molecular state, its activity would have been of the lower order. It is inferred, therefore, that the smaller knots were formed, in large proportion, of minute discrete particles, or of heavy molecules, and that these largely followed individual paths of the orbital type, instead of remaining in collisional interaction of the gaseous type.

While the projectile force, as well as the rotatory impulse incident to the expulsion of the eruptive belches, stood in the way of a direct concentration of the knots, it is assumed that their dispersive effect failed to prevent a certain considerable portion of the heavier material from remaining under the control of its own self-gravity. The existence of knots seems to imply this. This self-controlled portion constituted the real knot in the dynamic sense. Whatever part of the primitive bolt escaped from this control and scattered, was drawn into independent orbits about the sun and became planetesimals. Soon after emergence, then, most of the nebulous matter is supposed to have either been gathered into knots, the collecting-centers of the future planets, planetoids, and satellites, or into planetesimals, the food on which these knots subsequently fed.

Gathering these postulated results together, they form a graded series:

At the head were eruptive actions of the first order whose bolts were massive enough to retain strong gravitative control of themselves and be able to gather and hold very volatile as well as heavy matter. They hence had great powers of growth and became giant planets.

Next in order were bolts of inferior, but yet considerable, mass from which, at the high temperature of emergence, the most highly active molecules largely escaped, but the heavier and less active remained, and this selected portion constituted the material for the next stage of segregation.

Next below these came a much larger number of notably smaller bolts, most of them independent of the larger knots, some of them secondary to larger knots. In most, if not in all, of these, the mass was insufficient to hold atmospheric and hydrospheric material at the temperature of ejection from the sun, and rarely even at the much lower temperatures of open space. These, therefore, soon came to be merely swarms of heavy molecules and of aggregates; yet they had self-gravity enough to hold themselves in an assembled state.

Below all these there were the scattered products of dispersive action, embracing free molecules and small aggregates. These very generally were thrown into orbits of the planetary type and constituted planetesimals.

CORES OF KNOTS OF MEDIUM MASS

The status of the largest order of knots is clear, for they could hold the more active molecules, even hydrogen and helium. The status of the smallest order of knots was also clear, for they could not hold appreciable quantities of free nitrogen, oxygen, and water-vapor. The status of the knots of the intermediate order, that were to form the terrestrial planets, was more doubtful. There is little reason to suppose that the cores of the knots of the two smallest planets, Mercury and Mars, held, in a free state, any notable amount of the atmospheric gases, since the full-grown planets show only scant powers of holding material of such high molecular activity. Very likely they may have had cores formed of heavier molecules in a gaseous or semi-gaseous condition. The knots of the earth and Venus quite probably had the power of holding the atmospheric gases—if these escaped absorption or combination with the multitude of minute aggregates of the knots—after their temperatures fell to about that of their present surfaces. Earlier than this, when they were much hotter from recent emergence from the sun, their competency is more doubtful. The balance of probabilities seems to favor the view that the knots of Venus and the earth were formed chiefly of heavy refractory material assembled largely in an orbital state around a core of gaseous or semi-gaseous matter of heavy molecules and that there were mingled with both these parts considerable atmospheric material.

SPECIFIC ORIGIN OF SATELLITES AND SATELLITESIMALS

It is assumed as inevitable that the main ejected masses should have been attended by fragments or subknots torn from them as they were belched violently forth. It seems equally inevitable that there should have been different intensities of the eruptive force propelling the escaping bolt, beneath one part or another, so that rotation of the ejected mass was unavoidable. The extreme vigor of the expulsory action gives ground for supposing that the rotatory motion had appreciable value. The relations between the time and the movement of the tidal cones drawn forth by the passing star were no doubt important factors in determining the direction of rotation of the knots, for pulsations doubtless lingered in the spot from which the previous belch was shot and this affected the eruption that followed

FIG. 21.—An eruptive prominence of the sun showing ragged borders and a tendency to minor bunchings. Photographed at the Yerkes Observatory.

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While these fragments or sub-knots were shot out with the principal masses, it was inevitable that there should be divergencies which gave them separate courses near the main masses, which resulted in revolutions about these masses when their control was competent. They thus became attendant or satellite knots. Fragments whose courses were too divergent or too swift of course escaped. It is thus assumed that the collecting centers of the satellites-to-be originated as incidents of the vigorous ejection of the primary knots.

While the highest order of knots probably always had large hot gaseous centers, in spite of expansion and radiation on emergence from the sun, the outer portion of their spheres of control should have been occupied, in very open fashion, by ultra-atmospheres of molecules pursuing orbital courses, as set forth in the first chapter. Probably there were also minute aggregates derived from the nebula pursuing similar orbits in this outer portion. All these orbital bodies were of the nature of minute satellites, that is, satellitesimals, for they revolved about the centers of knots that were to form planets. Some notable part of the material of the planetary knots may have been made up of these satellitesimals. They probably served an important function in the evolution of the final state of the satellites and of the planets as well, for they were *dynamic*, as well as material, food for both.

While such a degree of specific postulation may not unjustly be regarded as pressing deduction—or speculation, if you please—to great lengths, this may perhaps be pardoned in so far as it gives a definite picture of the working essentials of the planetesimal hypothesis in its closest application to planetary origin, for, given these postulates, the rest of the evolution is predetermined by the mechanics of the case. These specific pictures are indeed little more than attempts to interpret the physical and dynamical vestiges now embodied in our planetary system by projecting these backward to their initial states. It goes without saying that these deductions are tentative, and that they will quite surely need emendation when the vestiges that are their source shall be better read and more successfully traced to their antecedent states.

AGGREGATION OF THE NEBULOUS MATTER

Three processes are believed to have formed the essential features of the concentration of the nebulous matter into planets, planetoids, and satellites: (1) the direct condensation of the gaseous centers of the knots, where there were such, into liquid or solid cores; (2) the less direct and slower collection of the orbital or satellitesimal particles of the knots into solid cores; and (3) the still slower gathering of the planetesimals into the knots or through these into solid cores.

1. The condensation of the heavy vapors of the smaller knots should have been direct and rapid. In the giant knots, condensation doubtless proceeded more deliberately, both because of the slower loss of heat and because

of the larger presence of highly volatile and uncondensable gases. In knots of the intermediate terrestrial class, the heavy vapors doubtless condensed rather rapidly, while the highly volatile and irreducible gases gathered into atmospheres about the nuclei or cores.

2. The gathering of the orbital part of the knots, the satellitesimals, into the planetary and satellite cores, was doubtless a slower process, for it depended, in part, on the collision of the satellitesimals with one another in their orbits—which would take place only in so far as these crossed, or were made to cross, one another—and, in part, on collisions with infalling planetesimals—which either drove them into the nucleus or into new orbits from which sooner or later a plunge into the nucleus would be likely to ensue. A planetesimal plunging into a satellitesimal would drive it into the nucleus in only a certain proportion of cases, but, under the laws of mechanics, both planetesimal and satellitesimal must return to the point of collision—unless diverted by some intercurrent agency or prevented by too high velocity—and be subject to another collision which would give another chance of being deflected into the nucleus, and so on indefinitely.

3. It appears then that the orbital or satellitesimal deployment of the particles of the knots served not only as a slow source of feeding for the nucleus or core, but also as a collecting mechanism for catching planetesimals; so also, in reciprocity, the planetesimals aided in driving in the satellitesimals. In some measure, indeed, the whole of the sphere of gravitative control surrounding each knot served as a collecting agency by reason of its power of deflecting passing particles toward the center

and thus increasing their liability to be caught. But the process of aggregation was none the less slow.

If now the earlier picture of the mode by which the passing star diverted the solar projectiles into orbital paths about the sun be recalled in its details, it will appear that the movements of knots and planetesimals were all in the same direction. A few of the satellite knots and satellitesimals might, indeed, have moved, and did move, in the opposite direction, but these are here negligible, though important in other relations. The fall of planetesimals into the knots came then from overtakes in paths that were similar. The impacts must often have been rather sharp, as we measure impacts in terrestrial matters, but they were relatively mild in the celestial sense, and the generation of heat as a consequence was relatively small. There does not seem to be any cogent reason for assuming that molten planets would arise from this mode of aggregation, except in those cases in which the knots were very large.

If we recall again the generation of the nebula, it will appear that, while all the orbits of the knots and the planetesimals were elliptical, there was much variation in the degrees of ellipticity and in the dimensions of the orbits. The orbits were hence generally unconformable to one another, in some degree, and the crossings of paths were many and various. These crossings were all the more prevalent because nearly all the orbits lay in, or near, a common plane determined by the passing star. These crossings were vital features in promoting aggregation. The fact that all the bodies in these intertangled orbits were moving in the same general direction and at rates not radically different gave time for gravitative action with minimum resistance to gravitative pulls. These conditions were also favorable for electric and magnetic action. These were liable to be more effective than gravitation in their effects on minute bodies. At present, too little is known of the electric and magnetic states in the heavens to warrant confident deductions, but it is prudent to recognize them as factors of probable importance.

The sweeping of the nebular haze into the knots was aided by the shifting of the orbits of both knots and planetesimals. This shifting arose inevitably from their mutual attractions, just as the orbits of the planets now shift by reason of planetary attractions. Thus fresh opportunities for collisions and close approaches were being evolved as time went on.

Furthermore, as the knots grew, the form of their orbits changed as a consequence of the accession of planetesimals. These new orbits cut the orbits of the remaining planetesimals in new ways with new possibilities of conjunction. Thus the process was helped on by the very results of its own progress.

However much the process of gathering the scattered matter into the planetary cores was aided by these auxiliaries, it was, as already remarked, slow. Inspection shows that the chance of any particular planetesimal falling into any given planetary nucleus was very small compared with the chances of escaping and continuing in its own independent career. In the alternative between a free life as a planetesimal and absorption into the larger life of a planet, planetoid, or satellite, the statistical allotment of chances was such as to give the larger share to the former in any given short epoch.

In the average case, the absorption was long delayed. This slowness of growth vitally conditioned the physical states of the planetary cores in their early history, a subject of peculiar interest in the case of the earth, which will claim attention in the next chapter.

EVOLUTION OF CIRCULARITY OF ORBITS

When a multitude of bodies moving in eccentric orbits of like type coalesce into one body, the resulting orbit is almost necessarily more circular than the most eccentric of the previous orbits; it is likely to be more circular than the majority of them; it may even be more circular than any of them. In sketching the origin of the orbits of the knots and planetesimals, it was noted that there was a progressive broadening of the elliptic orbits until their greater extensions came to be transverse to those of the first-formed orbits. Furthermore, there were two sets of orbits, one on each side of the sun. Developed in this way, there was a strong presumption that the coalescence of a vast multitude of planetesimals in building up each knot into a planet, planetoid, or satellite would give the mass an orbit much more nearly circular than either the average planetesimal or the knot had possessed before. The principles of such combinations are illustrated in Figs. 22 and 23. While these are selected cases and somewhat specially favorable perhaps, they yet fairly represent the tendency of aggregation to produce circularity.

In support of this, it is interesting to note that the orbits of the four great planets—which must probably have grown most—are more nearly circular, on the average, than those of the terrestrial planets—which should have grown less—and that the orbits of these, in turn, are more nearly circular, on the average, than the orbits of the planetoids which probably grew still less.

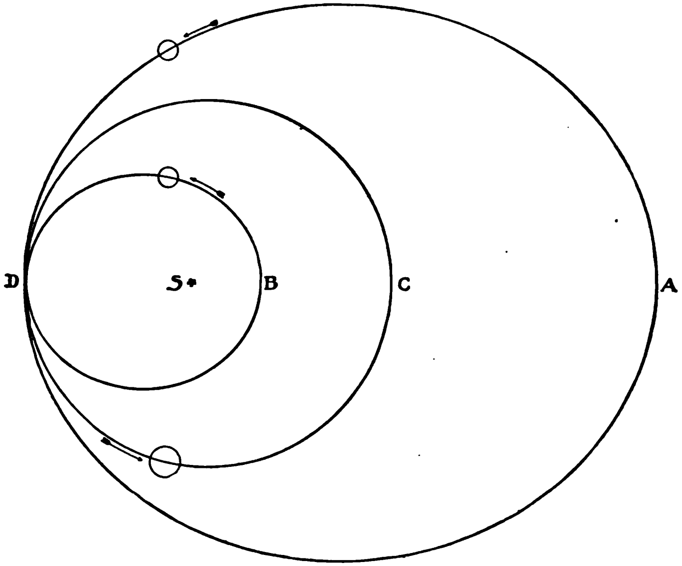

FIG. 22.—Diagram to illustrate the tendency toward circularity when the orbits of the uniting bodies have concentric positions. The two bodies revolving in the orbits *A* and *B*, and uniting at *D*, necessarily take an intermediate orbit, *C*, with an obvious advance toward circularity. In less simple and symmetrical cases, the result is less obvious, but it would be of the same order in all cases involved in the problem in hand.

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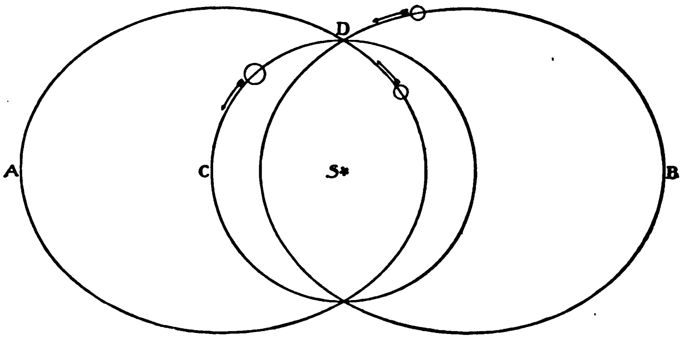

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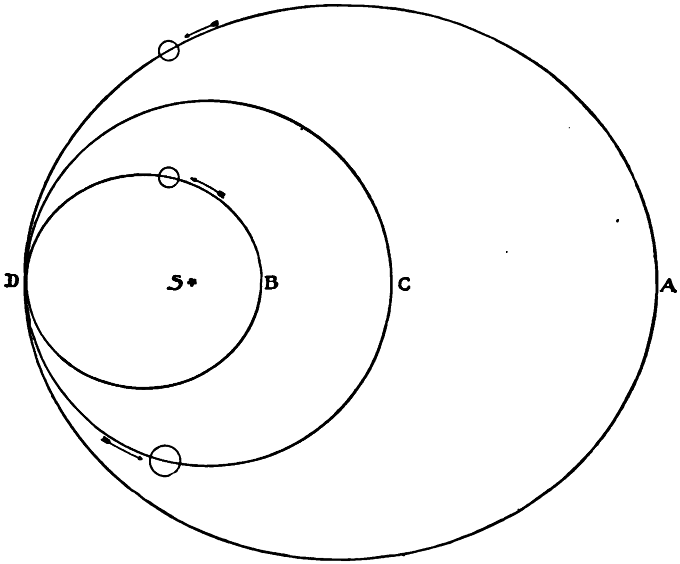

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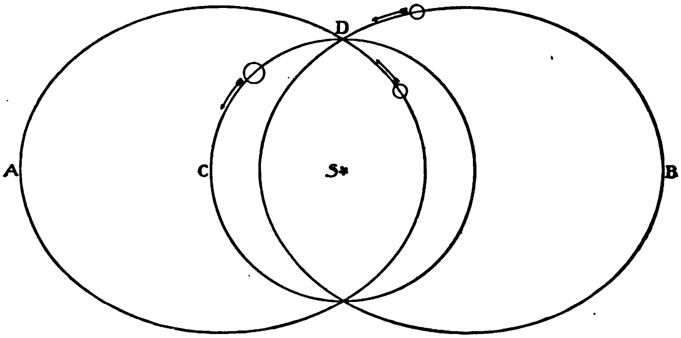

FIG. 23.—Diagram to illustrate the evolution of circularity where the orbits of the uniting bodies are as far as possible from having concentric positions. The bodies in the orbits *A* and *B*, meeting and uniting at *D*, necessarily take an intermediate orbit *C*, which is obviously more circular than either. In less symmetrical cases, the tendency would be of the same order.

If any planet grew most from planetesimals moving in orbits larger than its own, its orbit would have been enlarged; if it grew most from planetesimals in smaller orbits, the reverse. If, therefore, the planetesimals were evenly distributed, and if two of the planetary knots, at the beginning of their growth, happened to have orbits close to one another, while on the opposite sides there were wider spaces, the two knots would add less matter to themselves from the narrow space between them, for which they were competitors, than from the ampler space on the opposite sides where the feeding-ground was more largely their own. As a mechanical necessity, they would move apart. In the course of a growth involving probably more than half the total mass of the planets, this automatic adjustment should probably have had an appreciable value, modifying the original spacing of the planetary nuclei.

If Neptune grew from the knot that was projected farthest from the sun, there should have been little scattered matter beyond it to be gathered in on its outer side, while not a little may have fallen somewhat short and have been gathered in later on its inner side. In this case, the Neptunian orbit shrank as the planet grew, and this may be why its orbit falls far short of the position assigned it by Bode's formula.

GROWTH AND ADJUSTMENT OF THE SATELLITES

The growth of the satellites from secondary knots is presumed to have followed the same general lines as the growth of the planets from the primary knots, but certain special features call for comment. All satellites, as a necessity of their continuance as satellites, revolve within the spheres of control of their primaries. In view of this, it is assumed that the satellite knots left the sun within the spheres of control of their primaries and that they always remained within them. A captured satellite, while perhaps not an impossibility, should bear distinct earmarks of its exotic origin.

Each of the large families of satellites is assembled in a disk in or near the plane of the planet's equator. The configurations of these satellite families are very similar to that of the planetary family, with a single notable exception. There are no retrograde planets, and theory provides for none; but three retrograde satellites have recently been discovered, though theory makes them possible. In addition to these, there are the highly oblique satellites of Uranus and Neptune, but these perhaps belong to a somewhat different type. The rotations of the planets Uranus and Neptune are not known. Their satellites probably revolve harmoniously with them. However this may be, the satellite orbits are highly oblique to the invariable plane of the planetary system and may be regarded as retrograde. This attitude is perhaps assignable to the mode of escape of the parent knots from the sun. If these knots were the products of the first great eruptions from the opposite sides of the sun, their rotations were uninfluenced by prior great eruptions and the law of chance was applicable to them; this they fairly satisfy. The great belches that came later may have been much influenced by the pulsations that long followed the ejection of their predecessors. At any rate, the rotations of Jupiter and Saturn are forward and quite swift; they give no hint of instability or reversal. Both Jupiter and Saturn have goodly families of satellites that revolve in harmony with themselves. Yet Jupiter has two moons that revolve in a retrograde direction, and Saturn has one. Under the planetesimal view this is interpreted to mean that the larger part of the fragmenting action about the borders of the great belches that sent forth the Jovian and Saturnian knots was actuated by the great impulses that gave rotation to the primary knots, but yet that minor fragmenting outbursts occurred on the opposite side and gave rise to secondary knots with retrograde revolutions. These probably succeeded in maintaining themselves because they were far out where orbital matter was scant, and the amount

FIG. 24.—An eruptive prominence of the sun showing a series of sub-knots projected with the main knot. Photographed at the Yerkes Observatory.

they encountered was limited. As anticipated by Moulton, from the mechanics of the case before their discovery, their orbits are notably eccentric and the planes of their orbits divergent from the equatorial planes of their primaries.

These anomalies aside, the distinctly harmonious relations of the other satellites, the great majority, is so notable as to imply an efficient cause. The primary basis for harmony probably lies in the fact that the impulse which gave the planetary knots their initial rotations also gave the secondary knots their revolutions. The same impulse should also have determined the planes of revolution of the multitude of more minutely fragmented bodies that were driven forth by the same blast and, like the satellite knots, revolved about the planetary centers, that is, were satellitesimals. Beside such inheritances from the original ejection, there should have developed perpetually an orbital ultra-atmosphere about each competent knot in the manner set forth in the first chapter. This should have been a perpetual dynamical tie between the orbital portion of the knots and the cores, and should have constituted a harmonizing element at all times in the evolution. As the knots grew up in the midst of this satellitesimal system, and in this orbital ultra-atmosphere, and were fed from them and fed into them, their nearly complete mutual adjustment follows as a natural consequence. One feature of this adjustment was an evolution of the satellite orbits in the direction of progressive circularity as in the case of the planetary family; so similarly there should have been a spacing of the satellite orbits into similar sub-regularity. The genetic conditions made for harmony between the satellites that rotated ***with*** their primaries, but they worked in a diversive way with the recalcitrant ***retrograde*** satellites. It is of no little significance, in this connection, that Moulton foresaw not only that retrograde

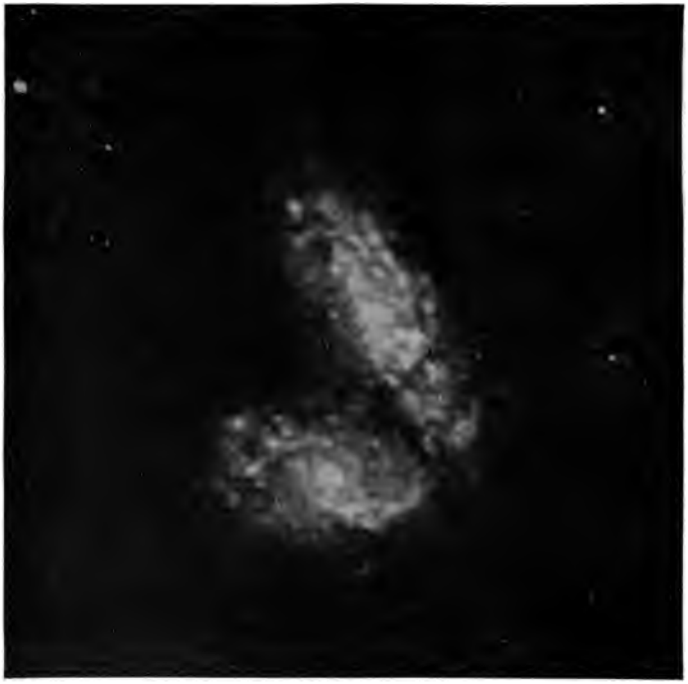

FIG. 25.—A double spiral nebula, N.G.C. 4567 and 4568. Photographed at the Mount Wilson Solar Observatory.

satellites might arise under the planetesimal hypothesis, but that their orbits would be more eccentric, and their orbital planes more divergent, than those of the concurrent satellites. This forecast, based on the natural workings of the mechanics of the case, has proved true in all the three cases of recalcitrant satellites since discovered.

PLANETARY ATMOSPHERES

From the beginning of the evolution of the planetary knots, theory indicates that there should have been an exchange between the orbital portion—the satellitesimals—that occupied the outer field of the sphere of control, and the denser inner parts that formed the more intimately organized collecting center; later, in the case of the earth and all larger planets, as the evolution perfected itself, this central mass gathered about itself a threefold atmosphere, a lower and denser collisional atmosphere, graduating outward into a vaulting or krenal atmosphere, and this, in turn, graduating into an orbital atmosphere, as set forth in the first chapter. This imposed a circulatory influence upon the whole assemblage within the sphere of control, except such parts as, from the beginning, were given an opposite régime and were able to retain a recalcitrant attitude. The main growth under these conditions tended toward circularity and symmetry.

Thus the planetary system and, in the main, the satellite system, grew more and more symmetrical and harmonious as the evolution of the spiral nebula went on, and so came, in time, to constitute a sub-circular disk of planets, themselves the centers of similar disks of satellites, in which, however, there remained diversities of masses, of rotations, of inclinations, of eccentricities, of space relations, and of other features, that, as dynamic vestiges, still betray significant individualities of origin.

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CHAPTER VIII

THE JUVENILE SHAPING OF THE EARTH

"As the twig is bent the tree is inclined."

The juvenile shaping of the earth may be said to have begun as soon as planetesimals commenced to plunge into the earth-knot of the nebula, and both knot and planetesimals began to gather into a dense body. The drawing of an atmosphere close about the young earth commenced almost simultaneously. The gathering of the primitive waters into the hollows of the earth-surface soon followed. These three concurrent activities were master-processes in the growth of the infantile earth; they were the geologic triumvirate. They wrought together toward the earth's final shaping into the lithosphere, the hydrosphere, and the atmosphere. Starting almost at the beginning of the earth's history, these three great activities gave direction to the planet's growth and thus dominated its later career. Our interest now centers in the way in which the young earth was forced to depart from an ideal spheroidal form and to take on deformative lineaments that grew at length into its present configurations.

As our genetic assumptions are now well understood, I beg to indulge more freely in the simple style of direct affirmation, when no new assumptions are involved.

SELECTIVE SEGREGATION OF HEAVY MATERIAL

As soon as the more refractory substances of the parent nebula began to gather to one another to form minute aggregates, these must have taken on different degrees of elasticity according to their several natures. As the collecting process involved collisions, the rebounds from the encounters became a selective agency, for the sharp recoils of the highly elastic aggregates were less favorable to concentration than the deadened responses of the inelastic aggregates. In these, the opposing components of motion were largely killed by collision, and, thus partially arrested, the aggregates responded more readily to gravity and were more largely drawn to the nucleus. Inelastic matter thus took precedence in assembling. Probably the metals—or their alloys, oxides, sulphides, and similar compounds—and the basic silicates were leaders in this early segregation. Obviously, however, no such selective process would be complete. At most, it led merely to a preponderance of basic and metallic material in the heart of the earth.

It is a growing belief that all great rotating bodies are magnetic because of the electric charges they bear. The magnetism of the sun, so brilliantly demonstrated by Hale and his colleagues, lends plausibility to this view, for the sun is too hot to retain the familiar magnetism of iron and its kindred metals. We have inferred that the nebulous matter of the terrestrial knot was rotatory from its very ejection from the sun. That it should have been affected by electric dissociation almost goes without saying. The luminosity of nebulae perhaps springs from their electric states. It is then a plausible conclusion that the earth-to-be was magnetic when it was only a nebulous knot. At any rate, the earth is now magnetic and we only follow the probabilities in projecting its present qualities back to its infantile states.

If magnetic, all planetesimal matter susceptible to magnetic attraction would have been drawn toward the earth-center by two forces, the magnetic and the gravitative. Iron, nickel, cobalt, and some of their compounds should have thus taken precedence over non-magnetic substances in entering the core of the earth.

Thus, the selective functions of elasticity and magnetism were perhaps united in concentrating a preponderance of heavy material in the heart of the earth. Nevertheless, this heavy material must have been much mixed with almost every other kind of material and the nucleus must have been heterogeneous. It should thus have furnished the basis for a profound inner reorganization; this will be the subject of the next chapter.

TEMPERATURE AND PHYSICAL STATE OF THE INTERIOR

What temperature and what physical state would have arisen in the heart of the earth from a slow planetesimal growth is a question of deep interest, but the final word is yet to be spoken. Much must have depended upon the extent to which orbital dynamics prevailed in the terrestrial knot. In deliberating on this question, it cannot be too constantly borne in mind that all but a small fraction of the earth-knot must have been composed of matter that condenses to the liquid or solid form at high temperatures. Even if this dominant refractory matter escaped cooling to the solid state as a result of the enormous expansion it suffered on emerging from the sun, it is doubtful whether, in so diffuse a state, it could have been maintained at vapor temperatures for any appreciable time after it began to sweep through interstellar space. The knots of spiral nebulae are apparently quite persistent, else they would be less common. If only sustained by inter-collisions of the gaseous type, they might be expected to collapse into a concentrated form in relatively short periods. It seems, therefore, a plausible inference that the orbital state prevailed in the outer portions of the knots. The interiors of the knots might still be gaseous.

Thus it appears that we cannot reason very securely relative to the primitive temperatures of the deep interior of the embryonic earth. It does not seem imperative to assume that the central temperatures were extremely high, whatever one may think of the probabilities of the case. It does not appear wholly safe to assume that even the innermost core of the earth took on the liquid state, even at the outset, or, if it did, that it long retained that state, however congenial to inherited predilections such an inference may be.

The conditions that controlled the accessions which constituted the later growth of the earth may be inferred with more confidence.

The vaporous nucleus that would directly condense may have been only a minor part of the knot; the primitive core may thus have been small. Yet it would be the mass of the whole knot that would be the selective agency in holding or failing to hold the several grades of molecules. If the mass of the knot bore any such ratio as we have supposed to the mass of the adult earth, it is safe to assign it gravitative power enough to have held free nitrogen, oxygen, carbon dioxide, and water-vapor. Let us proceed, then, on the assumption that planetesimals could reach the core of the earth only by plunging through an envelope of satellitesimals and atmosphere. At least, if an atmosphere were not present at the start, it would begin to gather at an early stage.

By far the greater number of the multitude of meteorites that plunge daily into our atmosphere become white-hot and are dissipated while yet in the very thin upper air. Only a very small proportion ever reach the denser air even; only rarely, relatively, do meteorites have mass enough to reach the earth before they are completely dissipated. Meteorites have, therefore, almost no power of heating the body of the earth. There is little reason, then, to suppose that planetesimals, even if they plunged through the satellitesimals and into the young atmosphere much more frequently than meteorites now do, could have greatly heated the earth. It is not likely that the planetesimals reached masses comparable to such of the meteorites as endure the atmospheric wastage and reach the earth's surface. If quite small, as seems probable from the mode of their formation, the planetesimals would be almost universally dissipated in the upper air, even though the young atmosphere was as thin as that of Mars. Even if this were not so, they would probably have been very cold when they entered the outer air and might have retained a cold interior, as meteorites sometimes do, notwithstanding the heating of their surfaces by atmospheric friction. Meteorites, even after they have plunged through the whole atmosphere and into the earth, are said sometimes to retain a very low temperature within. They are reported even to freeze the earth in which they imbed themselves. At any rate, the low temperatures brought in from space must be set over against the heat of atmospheric friction in the ledger of temperature effects. Very significant, in this respect, is the almost incredible existence of a small class of meteorites largely *formed of volatile and combustible hydrocarbons*. These have reached the earth without either complete vaporization or combustion. Meteorites (which are, in origin and in dynamic state, different from planetesimals, though the two are not uncommonly confounded) plunge into the atmosphere at velocities higher than those which probably actuated the planetesimals, for the latter moved *with* the earth and their mean differences of velocity were notably less. They should have joined it at perhaps a third or a fourth of the mean velocity of meteorites. The conclusion may therefore be drawn rather firmly that very little of the heat of impact of the planetesimals affected the interior of the earth's body.

The strokes of the planetesimals doubtless gave rise mainly to planetesimal dust in the thin air high above the earth's surface where such heat of impact as arose was readily dissipated. Settling thence gradually from the outer air as it did, there is perhaps as much reason to regard the planetesimal dust as a bearer of low temperature as of high temperature.

Leaving, then, as an open question the temperature of the innermost core of the earth—and so the question of its original state of fluidity or solidity—the matter added in the later stages by the fall of planetesimal dust and planetesimal residuals probably brought little accession of heat. The weight which was added by the accessions of course generated heat in the interior by compression, but the liquefying effects of this were antagonized by the increase of pressure that caused it.

A like remark may be made, with even less reserve, for the satellitesimal matter of the knot when encountered and carried down by the planetesimals.

From near its beginning, therefore, the earth is pictured as growing up largely by the accession of planetesimal dust after it had been wafted to and fro by the atmosphere. The accessions were solid matter. It is our view that they remained solid, except as specific conditions enforcing liquefaction arose after their burial and reduced selected portions to the molten state.

SOURCES OF THE ATMOSPHERE

The growth of the atmosphere of the young earth is assigned to three sources. A certain proportion of nitrogen, oxygen, and other atmospheric elements doubtless found a place in the nebulous knot at the outset. In part these elements were entrapped or combined with less volatile matter and were carried with it into the growing earth-body. Set free later by internal reactions, a portion of this atmospheric material was carried out by volcanic and other extrusive processes. Another portion of the atmospheric material of the knot probably escaped combination and entrapment and simply gathered more and more closely about the earth-body as it grew; this portion was merely the irreducible gaseous residuum of the knot. A third portion of the atmosphere probably came in with the planetesimals, or as planetesimals. Some of this alien portion may have been in a free molecular state as it came in, that is, constituted molecular planetesimals; more largely perhaps it had been combined, absorbed, or occluded in the planetesimals as they formed in space. Such absorbed, occluded, or combined constituents were doubtless largely driven out when the planetesimals were heated by their plunge into the upper air; for the rest, they were probably given forth after burial by internal processes. These sources of gain imply that there were means of loss when the nature of the action was reversed.

The atmosphere is hence regarded as the product of a long and complex growth, beginning early and continuing even to the present day. This long growth was conditioned, on the one side, by the rate of accession and by the nature of the added constituents, and, on the other, by the power of the young earth to hold the various kinds of atmospheric molecules that came to it, as also by the various rates of their combinations with earth-substance. The conditions that affect the power of the earth to hold an atmosphere were discussed in the first chapter. These conditions no doubt played a very important rôle in determining the ingredients and volume of the juvenile atmosphere. They have probably served an equally important function in their selective influence on the maintenance of the atmosphere during all the ages that have intervened since.

It is barely possible that the earth-nucleus, in its very earliest stages, did not have gravitative power enough to hold an atmosphere of such active constituents as free nitrogen, oxygen, and the vapor of water; it is barely possible also that these elements were all united with the matter of the knot; in either of these cases the primitive atmosphere was probably formed only of vapors whose molecular velocities were lower, and the later atmosphere must have come almost wholly from chemical dissociation and from the planetesimals and satellitesimals. But this seems, on the whole, improbable.

COLLECTION OF THE HYDROSPHERE

The gathering of the waters upon the face of the earth is supposed to have been somewhat delayed, at first, but yet to have soon joined the atmosphere in blanketing the globe. Molecules of water-vapor have somewhat higher molecular velocities than molecules of nitrogen, oxygen, or carbon dioxide, and hence an ocean may not well precede an atmosphere in a gradual process of growth. In any case; water-vapor should have preceded the liquid form. If the mass of the terrestrial knot were so great as 30 or 40 per cent of the grown earth, it should have had gravitative power enough to hold water-vapor, and, judging from the chemical constitution of the sun, a sufficiency of appropriate material should have been shot forth with the knot to form a hydrosphere as well as an atmosphere about as soon as the mode of aggregation permitted.

The special property of water responsible for the growth of great oceans is the persistency with which its vapor condenses to the liquid form at the temperatures that prevail at the surface of the earth. Free nitrogen, free oxygen, and most of the other permanent constituents of the atmosphere never take the liquid form under natural conditions of temperature and pressure. By virtue of its prompt condensation, water becomes a very unstable constituent of the atmosphere and rarely forms any great proportion of it. The liquid state is water's normal form under most terrestrial conditions. None the less, water-vapor was doubtless the parental form of the whole hydrosphere. If water-substance were to be gradually removed from the earth, water-gas would linger after liquid water had disappeared; so, by reversal, it quite certainly appeared before water condensed. At all times after their inception, the water-vapor enveloping the earth and the waters on its surface, strove to maintain an equilibrium between themselves, but this was greatly embarrassed by constant changes of temperature, and was furthermore much interrupted and restrained by the circulation of the atmosphere, and so the juvenile earth undoubtedly had its arid regions as well as its humid regions. The old picture of a warm moist atmosphere enveloping the whole earth is held to be physically untenable even under the conditions of the early geologic ages, for descending air currents in a natural circulation are inevitably dry currents.

CO-OPERATION OF LITHOSPHERE, HYDROSPHERE, AND ATMOSPHERE

Our general picture of the history of the juvenile planet is then tripartite, a small lithosphere, a small atmosphere, and a small hydrosphere growing up together in co-operation and at the same time in competition and antagonism, and so, by their interactions, working out their adjustments to one another progressively. The environing conditions are not regarded as radically different from those of the geologic ages, or of the present. Notable oscillations, however, ran through the whole history. The picture is quite at variance with the traditional one sketched in the Introduction.

The story of the interactions, the competitive struggles, and the mutual adjustments of the great triumvirate in progressively shaping the lithosphere, the hydrosphere, and the atmosphere throughout the ages is the task assumed by geology. The great contest, however, had its origin in the genesis of the earth, and the main lines of the contest took form in the earth's infantile ages. We should fail to tell a vital part of the story of the earth's beginning if we passed without note the inception and the early alignment of the prolonged struggle that is the very soul of geologic history.

At the stage when first the stratigraphic record becomes distinctly legible, the struggle of earth, air, and water had attained working relations much the same as they bear today. The lithosphere had taken on great continental reliefs and great oceanic depressions. Then, as now, apparently about two-thirds of the surface was deeply submerged and about one-third was strongly bowed upward. The waters sometimes confined themselves to the abysmal depressions and sometimes flooded the lower borders of the continental protrusions. The waters and the air joined forces in a ceaseless endeavor to wear down the protrusions, but they rarely more than half succeeded before the accumulated stresses of the lithosphere brought on a new series of deformative readjustments by which the continental protrusions were renewed, the basins deepened, and the waters withdrawn more largely into the abysmal depths. The powers of the lithosphere, though only now and then made manifest in marked degree, have thus far held the mastery. The final issue hangs no doubt on the hidden resources of renewable reshaping power in the lithosphere. If these resources shall fail, the unceasing gnawings of the rains, the winds, the streams, and the seas will certainly cut away the land and leave a universal ocean, save as volcanic piles—if the volcanoes still live—may rise above its surface. If the hidden powers of the lithosphere shall seriously weaken with time, the periodic incursions of the sea will disastrously constrict land life at the epochs of crisis. But if the hidden powers of the lithosphere are adequate to indefinite renewal, continued rejuvenations of the continents may be assumed to be assured. The ultimate issue hangs on the reserve power of re-formation—we say deformation—inbred in the lithosphere in its youth. From this the renewal springs. The nature of these inbred resources is now our problem.

Let us recall the processes of growth that gave the basal elements of this inbred power, as interpreted by the planetesimal hypothesis. The earth-body grew up by a multitude of minute accessions coming by way of the atmosphere and carried by it to resting-places far or near as conditions determined. The atmosphere thus took the first turn at the distribution of the earth's new material. If the accessions fell into the waters, these took the next turn and bore the accessions along their courses to resting-places which they determined. The lithosphere was permitted to receive its accretions only as dictated by these intermediary agencies.

But, on the other hand, the configuration of the earth-body had previously determined where the waters should lie, and, in some measure, how they should move, and had thus circumscribed, by its own prior action, the water's control over the accessions. In a less obvious way, the configuration of the earth's surface had given shape and place, in some degree, to the great swirls and gyrals of the atmosphere, and, to that extent, prescribed the control of the first controller of the planetesimal dust. Here is a tangle of contesting agencies most forbidding to the inquirer who would read their history in their results.

The task is further embarrassed by interchanges of cause and effect. Each cause is no sooner realized in an effect than this effect becomes a new cause, and so the chain of sequence runs indefinitely on. Not only this, but an atmospheric cause gives a result in terms of hydrospheric or lithospheric phenomena, and this result, becoming at once a cause, may either yield a result in its own field or in the field of either one of the others. Thus there arises an intricate intertanglement of antecedents and consequents that are in a sense interchangeable.

To trace back to their initial stages the contribution of each agency is scarcely less than a hopeless task. To start with a hypothetical cosmogonic status and trace forward the special modes of action and the values of participation of each of the three great factors, deductively, is scarcely more hopeful. To try to do both and to shape each to fit the other may seem to savor of selective adjustment and mutual accommodation, but it is legitimate if used merely as a means of guiding deduction by observed consequences, and of guiding induction by discernible antecedents. The mutual suggestiveness of such a reciprocal method gives it the meager promise which alone the entangled case affords. It will no doubt take years of persistent trials to work out the full chain of causes and effects that led to the results revealed in the adult earth. But the longer the road, the sooner a start were best made, however falteringly.

SHAPING AGENCIES

The lord of the shaping agencies was obviously gravitation, whose ideal product is a perfect sphere, the standard from which deformations are measured. By far the most powerful of the deforming agencies was rotation. In the course of the earth's past history it has imposed on the ideal earth-sphere dictated by gravitation a series of depressions of the high latitudes—interrupted probably with many intervening elevations—and a series of swells of all the low latitudes—interrupted probably with many intervening depressions—the whole series resulting in a graduated polar flattening and a graduated equatorial bulge, technically a zonal harmonic deformation of the second order. The residual values of all these past deformations now appear as differences in the earth's radii ranging up to 13.4 miles (21.6 kilometers). The deformations that most nearly rival this in magnitude are the continental swells and oceanic sags. The vertical swing of these is less than half that of the rotational effect, while the amplitude is also very much smaller. Even the utmost range of mere detail measured from the highest mountain peak to the lowest known sea-deep, falls short of the broad rotational range, while the masses involved in the peaks and deeps are trivial compared with those of the harmonic rotational deformation.

In addition to its marked superiority in potency, rotation quite surely took precedence in doing its deformative work; it was an active force at the very outset;

very likely it was more effective in the growing stages than afterward. In our picture of earth-genesis, rotation co-operated with gravitation even in giving the core of the earth its initial form. The deformative work of shrinkage and similar agencies could only have come into effect later. The initial departures of the earth from an ideal sphere are therefore assignable chiefly to rotation.

The simple harmonic swell at the equator and flattening at the poles need not, however, detain us; this is not the special point of interest, but rather the *secondary* deformative effects, commonly altogether neglected. These arise from the adjustments that are forced whenever there is any appreciable *change in the rate of rotation*. It is obvious that if the rate of rotation is increased, the high latitude areas must sink, and in so doing suffer mutual crowding and compression; while the low latitudes must rise and suffer tension; and vice versa, if the rotation is slackened. It is the effect of such changes on the shape of the juvenile earth that requires consideration. There is need, therefore, first to consider the nature of the changes in the rate of rotation that probably arose in the early history of the earth, and then to study their deformative effects on the globe.

Let us note, then, at the outset that the present rotation of the earth is only the outcome of a long history; the net result may be far from a true index of the sum-total of rotational effects of all past time. This total value it is impossible to estimate in more than the roughest way, even if the planetesimal theory, in the venturesomely specific form we have given it for just such trial purposes, be fully accepted; but a review of the assigned conditions may be helpful toward a tentative picture of the case.

The initial rotation of the earth-knot is assigned to the inequalities of the impulse that shot it from the sun and to the inequalities of resistance to its escape. Deductions from the nature of the case, as well as inferences from the observed outlines of nebular knots, favor an orbital deployment of the knots in some large degree at least, and this implies much rotatory momentum at the start.

But the critical element probably lay in the effect later produced by the infall of planetesimals as they gradually built up the earth nucleus into the adult planet. Near the close of the fifth chapter, attention was called to the reasons for believing that the effects of the infalling matter tended toward *an equilibrium value.* This effect should have tended to reduce any inherited rotation to the equilibrium value and to maintain *an equilibrium rate of rotation* based on that value. It was pointed out that the mechanism would tend to keep the rate of rotation oscillating about this equilibrium value. The planetesimals were very irregularly dispersed along the arms of the nebulae, and the knots were unsymmetrically placed relative to the nebular streams, so that, in spite of all smoothing out of effects by the compensations of chance distribution, the net effects must have been inconstant, and the rate of rotation must have oscillated about the equilibrium rate. It was, however, pointed out that the equilibrium rate itself was almost certain to undergo changes with the alteration of basal conditions that arose from the progressive ingathering of the planetesimals. Thus, even after an equilibrium rate was once reached, the whole history of rotation should have been one of oscillation, and oscillation should even have marked the progress toward the equilibrium rate.

Superposed on the deformative effects of rotation were those that sprang from the tides and from the shrinking of the earth-body. The former were doubtless slight, but the latter were important. As the earth was built up by the accession of finely divided matter laid down by air or water, and as this loose material was progressively buried, there inevitably ensued much compression and much molecular reorganization to secure greater density. While the condensation was no doubt progressive, it probably obeyed the higher law of pulsatory action and fell into the periodic habit that so notably marked the well-known stratigraphic ages. In any case, the shrinkage tended to accelerate the rate of rotation; if periodic, the acceleration was periodic. Episodes of acceleration from shrinkage are pictured as occurring at intervals all through the growing stages of the earth even more frequently than in later times, and they must have tended to raise the speed of rotation above the equilibrium rate and incite a tendency to restoration in which the infalls and the tides would co-operate.

It appears, therefore, that changes of rotation were the order of the day during the growth of the juvenile earth. In the known geologic ages, the effects of growth are negligible, but there continues to this day a contest between tidal retardation and its allies, on the one hand, and shrinkage acceleration and its allies, on the other.

From recent exact studies of the moon by Brown[1] and others,[2 and 3] and from comparisons of the moon's irregularities with those of Mercury and Venus, there appears to be some ground to suspect that even now there are changes in the earth's rotation that may be astronomically detected. In view of the shortness of the observational periods available, this is more than could confidently have been expected, but if it shall be realized, it may be regarded as quite in harmony with genetic and geologic antecedents.

If the rate of rotation has changed often, the vital question arises: How did the earth accommodate itself to the compressional and tensional stresses that inevitably accompanied the reciprocal swellings and sinkings of polar and equatorial regions? Obviously this would depend, so far as its precise manner is concerned, on the physical state of the interior. Consistently with what has been said before, and conformably with the virtual demonstrations of the solid state of the interior, our discussion will proceed on the assumption that the earth, at all stages of final shaping, was an elastic solid of high rigidity. We shall pass lightly all consideration of the innermost core of the earth on which a final opinion may wisely be held in abeyance. We shall, however, class it, by implication, with the rest, for unless there are cogent reasons for assigning it a different state, the most conservative view, pending further seismic data, is to assume that it followed the habit of the main body of the earth. The mechanism of extrusion, which we shall later discuss, would probably force a solid condition upon it, whatever its original state. The recent tidal determinations of Michelson[4] and colleagues[5] seem to imply such a state.

DISTRIBUTION OF STRESSES

The dynamic basis for deduction is a vital feature. It cannot be too thoughtfully considered that the stress-differences generated by changes in the earth's rate of rotation, according to Sir George Darwin,[6] not only pervade the whole interior, but increase progressively from surface to center where they are eight times as great as at the surface. There is a curious variation from a simple gradation of stress-difference from the center to the surface in the sub-polar portion, as shown in the accompanying diagram (Fig. 26) reproduced from Darwin's classical discussion, but that may be neglected here.[6] The internal stress-differences of the tides have the same distribution, according to the same high authority. So does any weighting or relief of load that takes the form of a belt about any equator of the globe with polar areas of the opposite state.

If resistance to movement under rotational stress were equal at all depths—or were easiest in the central parts, by reason of a mobile state—adjustment movements in the central parts would, we infer, take precedence, because the stress-differences are there greatest. Equal facility of movement would obtain when the resistances were distributed according to the same law as the stress-differences, in this case a gradation of resistances from eight at the center to one at the surface. The high-pressure results of Adams,[7] Bridgman,[8] and others show a relatively rapid increase of rigidity with increase of pressure. If an extrapolation of these to the heart of the earth could be trusted, the relative resistances to deformation there would be much higher than the relative stress-differences arising from change of rotation. But even if there were no doubt as to the continuance of the high rate of increase of rigidity, the conclusion would perhaps only apply to the average mass; very likely it might not apply to special tracts where cleavage lines, schistosity, and other facilities for movement had been developed previously by easement movements. It is assumed that structural adjustments to movement adequate to meet the demands of stress-differences of a high order arising from rota-

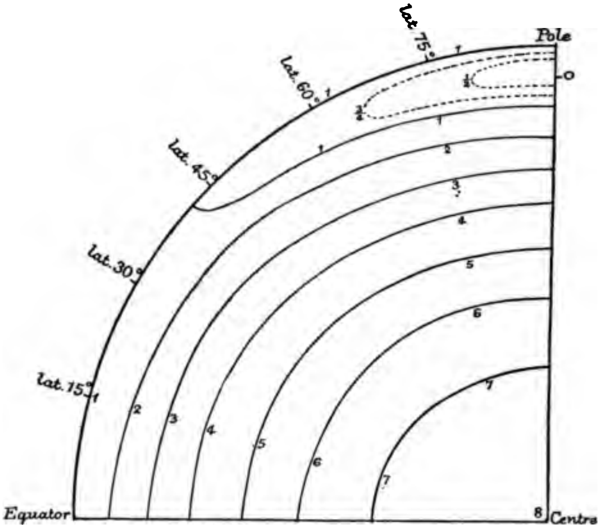

FIG. 26.—Diagram showing curves of equal stress-difference due to the weight of second harmonic inequalities or to tide-generating force. (Darwin.)

tion were developed in each stratum soon after it was added at the surface, and that these structural and textural features were retained, or were renewed as new requirements demanded while they were in process of burial to greater and greater depths. Otherwise that nearly perfect adjustment to rotational demands which the earth now exhibits, and which, judging from the distribution of the waters in the past, has prevailed throughout geologic history, would probably not have been realized. This is closely locked up with the question whether the crystalline structure developed near the surface would be retained as the layers were progressively buried under greater and greater weight. So far as both geological and experimental evidence goes, increasing load *favors* crystallization in rock substances, except in the extremely few cases in which crystallization involves expansion, as in ice and in bismuth. Since increasing load favors recrystallization into a denser form, there is a strong presumption that, as surface layers were more and more deeply buried, there would be a series of recrystallizations in the interest of increasing density. So probably there would be chemical recombinations of like import. All this looks not only toward continued but toward progressive crystallization with increasing depth. It seems inherently probable that such crystalline assemblages of molecules of like kinds, or of molecules mutually suited to one another, as serves the interests of compactness and order near the surface, would continue to obtain, abetted by adaptive changes, in the depths of the earth. This seems to be in close accord with the tenor of such specific evidence as is applicable to the case.

INTERNAL MOVEMENT

With little doubt, our best guide in forming a picture of the probable mode by which adjustment movements were carried into effect in the deep interior is to be found in the method by which the deepest accessible rocks have accomplished similar adjustment movements. In our view, the adjustment tracts were mechanically selected in each stratum, or group of strata, while still near the surface; it was then that deformative force was first brought to bear upon it; the resulting structures and textures were slowly carried down to deeper horizons as burial proceeded, and only progressive changes are to be considered. The initial conditions were those of ordinary diastrophism near the surface. The only special question is whether the structures and textures inherited from this sub-surficial diastrophism would remain serviceable for movement as weight was added, and would remain susceptible of further structural evolution such as would be serviceable for movement under the increasing pressures and stress-differences. This is closely tied up with the question whether crystallization can extend to the depths of the earth, a subject touched a moment ago. In addition to the considerations there urged, it may be added that this question receives a probable answer in the transmission of the distortional phase of earthquake waves through the deep interior, as recorded by seismic instruments. The transmission of distortional waves implies an elastic solid condition. In the core of the earth, the transmission of distortional waves at accelerated rates, through more than half the volume of the earth, probably implies specifically the continuance of crystalline structure to a like extent, at least, since the crystalline structure is the typical form of elastic earth-substances. The few exceptional cases in which "undercooled liquids," such as obsidian and other glasses, show facility in transmitting distortional waves can hardly have any bearing on this problem, since the hot interior of the earth is not a suitable environment for "undercooled" liquids. For depths that involve more than half the volume of the earth, the speed of the distortional waves increases notably with the depth, which implies that elastic rigidity rises faster than density, for the latter tends to reduce the rate of transmission. Toward the center of the earth a change seems to take place and the interpretation of the seismic waves is embarrassed by uncertainties due to imperfect or insufficient data. Pending better data, we prefer to ascribe the change in the seismic waves to a change of earth-substance from predominant silicates in the outer part to predominant alloys in the heart of the earth. The high specific gravity of the latter should damp the speed of the waves and might naturally induce troublesome refractions.

It seems, therefore, most in line with the trend of the results of recent investigations to assume that a crystalline state extends far down into the depths of the earth, if not to its very center. So, also, it seems most in the line of probabilities to assume that movements forced on this crystalline mass by stress-differences in the deeper horizons will follow the methods that obtain in the lowest horizons laid bare for study by denudation and diastrophism; or if not by these precisely, by supplementary methods of like nature applicable to the crystalline state.

MODE OF INTERNAL MOVEMENT

Now the chief mode by which solid rocks under high pressure and heat adjust themselves to great stress-differences is by recombination and recrystallization, as brought out by recent investigations in the field and in the laboratory.[9-14] By chemical and physical recombinations and rearrangements, the rock material is *reshaped into crystalline forms adapted to the imposed movement.* The change seems to be effected by free molecules acting individually, or in groups, only a small portion of the molecules undergoing change at any one time, the rest retaining their fixed adhesions, so that the mass, as a whole, remains essentially solid all the time, and may be quite rigid at all stages. In general, the mass does not seem to become even viscous. The relatively few molecules that are changing their adhesions and combinations at any instant, or are leaving one crystalline attachment and finding a new one better accommodated to the urgencies of the stress, seem to have, for the time being, a freedom of the fluidal type. Just how they make their way from one point to another among or through the crystals that form the prevailingly solid rock is undetermined, but the fact seems incontestable. The observed result is the formation of new crystals, or reshaped crystals, in parallel adjustment to the lines of movement. Platy and columnar crystals are induced so far as the nature of the material will permit, and their parallel arrangement gives rise to cleavage between the crystals in the direction of movement. This parallel orientation of the crystals carries with it the crystal's own internal cleavage. The total action is conveniently called "rock-flow" or "solid-flow," but it is not to be confounded with liquid flow or viscous flow. The textural result is a schistose tract. The plane of schistosity is in general accord with the direction of movement which is in the line of least resistance. In the case in hand, the lines of least resistance are dominantly directed toward the surface, and the schistose tracts should be vertical. This is not in contravention of the tendency to horizontal or low-angle schistosity in certain surficial or subsurficial situations where the dominant arrangement of forces is different.

It is therefore assumed, as the safest tentative working basis, that in whatever tract the stress-differences came to be most intense, recombination and recrystallization took place in the yield zones by the individual movements of freed molecules, with incidental cleavage and schistosity, and that the requisite movements and adjustments were thereby accomplished. It may be noted that, if this method became inadequate at any time, or in any place, the parallel planes previously generated were fitted to facilitate forced movement of a more intensive mechanical sort.

It is worthy of noting here that, if this method of stress-easement is not available in the heart of the earth, it does not necessarily vitiate the more essential features of the interpretations that follow. However, no limits to the process of crystallization and recrystallization are known under interior conditions short of melting and mutual solution. These latter probably contributed to the deeper movements more largely than to the more superficial ones, but the prevalence of liquid material seems to be rather markedly restricted by the evidences of elastic rigidity.

Granted that this mode of accommodation was available for stress-easement in the tracts where stresses were concentrated, the intervening masses were left relatively free to develop such higher degrees of rigidity as the imposed pressures might have required.

INTERNAL MOVEMENTS REQUIRED BY ROTATIONAL CHANGES

The shifts of matter within the earth required to reshape it to fit a new rotational rate are easily pictured. If rotation slackens, the equatorial tract tends to sink and suffer compression, while the polar portions tend to rise and suffer tension. Between the rising and falling tracts lie fulcrum zones in which there is neither rise nor fall. Across each fulcrum zone, however, there must be a shift of matter sufficient to relieve the sinking equatorial tract and supply the rising polar tracts. These fulcrum zones lie not far from 30° Lat. N. and S.; their precise positions vary with the degree of oblateness; but we need not dwell on these refinements. If rotation increases, the shifts are reversed, with reversal of compressional and tensional effects.

If the earth had a fluid interior, the main shifts would be made by flow in the true sense, but compressional and tensional effects in the crust would be felt and the deductions that follow would probably hold in a modified form. But if the earth is solid, with increasing rigidity toward the center, as growing evidence seems to imply, the shifts will tend to take place by massive movements that involve the least possible strain throughout the body and that throw as much of the deformative action as possible on zones of easiest yield. The number of lines of deformation will tend toward a minimum, so far as these can ease the stress. It is fairly safe to assume that the simplest segmentation of the body of the earth that would accommodate the main stresses imposed by changes of rotation was that most likely to have been adopted. Minor and more local stresses should have been eased by supplementary lines restricted to the lesser demands.

PRIMARY SEGMENTATION OF THE EARTH-BODY

In seeking the simplest mode of accommodation to rotational requirements, it may be first noted that, at the surface at least, where the action probably started, tensional stresses are easier relieved by disruption than compressional. If the rotation inherited from the earth-knot was greater than that of the equilibrium rate imposed later by the infall of planetesimals, as seems probable, the net tendency in the early ages would be toward slackening rotations, and hence toward compression in the equatorial belt and tension in the polar tracts. It is probable, therefore, that segmentation began under tensional conditions near the poles—where the stresses would be twice as great as at any given point on the equator—and that the other primary lines of accommodation developed from these initial ones. A like inference is to be drawn, even if segmentation did not start until after the rate of rotation had reached a stage of oscillation about the equilibrium value, for the tensional stage would act under less stress than the compressional and so would be likely to precede it, defining the lines of accommodation. But the order of precedence is probably not at all vital; it is, however, convenient to follow a definite line of interpretation, and the most probable line is preferable, even if it is not important.

Fortunately, the earth gives very instructive illustrations of how tensional stresses are relieved. The most illuminating example, especially for our purpose, is the mode of partition followed by certain basic lavas as they solidify, and shrink in so doing. The result is a columnar structure, well shown in the basaltic columns of the Giant's Causeway, Fingal's Cave, the Devil's Post Pile, and numerous other cases. These examples are the better because they relate not only to crystalline rock—the material under discussion—but to as representative a class of rock as could be selected. Under the shrinkage tension of cooling, the rock, as it forms, parts along planes that radiate from the points where the greatest tensions have been developed. *The parting planes are normally three in number and these diverge at angles of about* 120° (see Fig. 28). As these planes are extended, they intersect one another and divide the whole mass into six-sided columns. It is a matter of special interest that the edges of the columns are habitually raised, as shown in Fig. 27, and the center depressed. At the angles there are sometimes specially raised portions as shown in Fig. 29, giving what has been called a "ball-and-socket" structure. All these special features appear in modified forms in the application to the earth-body, as we shall see.

This classical case embodies the principle that where tensional stresses are concentrated about a given point or column, the most natural mode of relief is a partition of the mass into three sub-equal parts radiant from the point of greatest stress. There is, not unnaturally, much variation in the actual divisions of nature, as shown in mud cracks in particular, where the working factors are variable. The process is simply a mechanical accommodation to existing stresses and the result depends much on the homogeneity of the material and the uniformity of distribution of the stresses. It is not a crystalline process and the forms produced by it are quite lacking in the refined accuracy of crystalline structures.

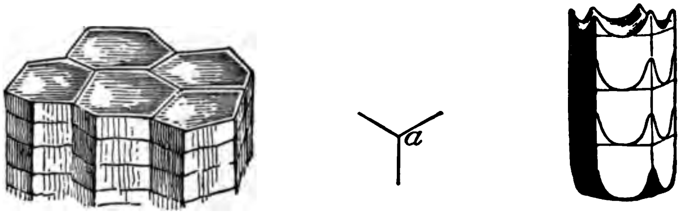

FIG. 27 FIG. 28 FIG. 29

FIGS. 27–29.—Fig. 27, sketch of sections of basaltic columns from Giant's Causeway on the coast of Ireland. (Chamberlin and Salisbury *Geology*, I, 476); Fig. 28, diagram illustrating the system of partition in the first stages of the formation of basaltic columns. (Chamberlin and Salisbury, *Geology*, I, 476); Fig. 29, diagram showing not only the raised edges of the columns (shown also in Fig. 27), but special elevations at the angles, giving "ball-and-socket joints." (After Scrope.)

Now in rotational accommodation, the tensional stresses that probably took precedence in the polar regions and were most intense at the poles would, under this law of partition, be eased most naturally by three fissure tracts radiating from the poles at angles of the general order of 120°, but no doubt varying considerably from the ideal. These radiating fissure tracts should theoretically be terminated at the fulcrum zone, for beyond that the stresses would be reversed. The effect would be to divide the circumpolar areas into three great segments of triangular form with their apexes at the pole and their bases on the fulcrum zone.

Now each polar unit must have acted reciprocally with an equatorial unit of the same value in any rotational change. It seems obvious that, if three great triangular segments had been thus defined in the polar regions, each reaching to the fulcrum zone not far from 30° latitude, the simplest, the most symmetrical, the most natural reciprocal working mates for these would have been three similar triangular segments set in reversed positions with their bases on the fulcrum zone and their apexes in the opposite direction. They would then be peculiarly fitted to seesaw across the fulcrum zone, and that was the nature of the motion required in response to changes of rotation. The apex of the equatorial triangles would thus fall in the fulcrum zone of the opposite hemisphere. The pairs of triangles lying base to base, each wholly in one of the reciprocating tracts and each under stress to yield what the other demanded, almost ideally fulfil the requirement of the case. The flexures in the reciprocating triangles would not, to be sure, be quite alike, but their working values would be the precise complements of one another.

Geologic history affords evidence that the two hemispheres have taken precedence in opposite lines, the southern in downward movement resulting in prevalent seas, the northern in upward movement, relatively, resulting in prevalent land. This seems to have been a fixed secular difference from Archean times to the present; it gives ground for supposing that one hemisphere, or the other, took precedence in the tri-segmentation just described. Whichever it was, the divisions in the other hemisphere would have been partially defined by such precedent action, and this partial definition should have guided the completion of the definition. Now the three pairs of reciprocating triangles developed from the pole that took precedence appropriated half the equatorial belt, in saw-tooth fashion, leaving the other half already defined in reversed saw-tooth form to mate with similar reciprocating triangles developed from the opposite pole, only the polar lines being needed to complete their definition. This done, the globe would be divided into six working pairs of triangular segments, the salients of the three in one hemisphere dovetailing neatly into the re-entrants of the three in the opposite hemisphere. The zigzag division of the equatorial belt had the advantage of giving it superior flexibility. As a simple adaptive working device, this partition seems to admit of no rival. Each triangular pair formed a quadrilateral, and this term will be convenient in tracing out the topographic results, since, in the main, each working pair gave a common physiographic product, though there was some tendency to division also along the fulcrum zone.

It has been convenient to sketch this divisional process as though it were superficial, but we must hasten to observe that the deformation involved the whole earth. The rotational stresses extended to the heart of the earth and grew in intensity in that direction. The sides of the reciprocating pairs of triangles, or the quadrilaterals, are to be pictured as extending to the center of the earth. Thus extended, they constitute four-sided pyramids with their apexes at the earth's center. Each adjustment to a new rate of rotation may then be pictured as a north-south swaying of these pyramids on their apexes attended by the requisite up-warp and down-warp of the reciprocating halves, and this picture is peculiarly appropriate to a body predisposed to move as a solid mass. This seems to be the simplest mode of adjustment available for such a reciprocal deformation in such a solid spheroidal mass. Its simplicity and its adaptation to its special function are perhaps its strongest credentials.

ADAPTATION TO TIDAL ACTION

This segmentation of the solid earth-body lends itself happily to the feeble tidal deformations that were forced upon the earth in constant bi-daily succession. The internal stresses of the body tides have the same distribution as those of rotation, according to Sir George Darwin.[6] While the tidal cones are constantly shifting northward and southward, their mean position is astride the equator. In this position they reach to about 60° N. Lat. and 60° S. Lat. At the poles the mean effect is perpetual depression; this would be constant if the tides did not shift in latitude. As pointed out in the study of solar eruptions, the tidal cones represent the lifting effect of the tidal forces. As they sweep about the equator, in their semi-diurnal courses, the equatorial ends of the diamond-shape segments rise and fall through nearly the maximum amplitude of the tidal effect, while the mean movements of the polar ends are low and at the polar angles theoretically zero. The mean oscillations of the east and west sides are intermediate in value. These differences of movement are probably exaggerated by the relations of the two ends of the oscillating segments. The polar ends are snugly wedged together and mutually aid one another in approximating zero movement. The equatorial ends join their partners of the opposite hemisphere along a zigzag line of notable flexibility which offers much larger opportunities for accommodation to mutual motion. The tidal movement involves an internal tortional strain in each segment and yield takes place with greatest facility in the equatorial ends. Recent tidal studies at Potsdam by Hecker, and, more completely and decisively, at Lake Geneva by Michelson[4] and colleagues, have shown that the earth-body yields more in a north-south direction than in an east-west direction. This suggests that perhaps the segmentation imposed on the young earth by rotation, and periodically revived ever since by changes in rotation, by tidal strains, by the indirect effects of shrinkage, and other agencies of deformation, may offer the mechanism out of which this difference grows, in whole or in part. Reciprocally the phenomenon of superior north-south flexibility lends support to the suggestion already advanced that easement zones of rather free yield were developed by the repeated movements imposed by the early rotational stresses and that the mild stresses of the body tides, the rotational stresses incident to shrinkage, and perhaps the stresses incident to general loading and unloading have served to keep these in working function.

EMBRYONIC FRAMEWORK OF THE INFANTILE EARTH A BASIS OF GROWTH

If the earth were segmented in this way to accommodate the recurrent stresses imposed by changes of rate of rotation, abetted by the semi-daily pulsations of the body tides and by periodic shrinkage; and if the main easement zones lay between the quadrilaterals so defined, the fissurings, faultings, foldings, and other special features of deformation forced by the tensions and compressions of the required readjustments would chiefly lie along the segment borders and the general effect would be elevation, and in general the elevation would be greatest at the angles, where three lines of disruption join. In this there would be close accord with the surface shaping of sides of basaltic columns which are raised at the edges and especially at the angles, as shown in Figs. 27 and 29. The easements of secondary stresses would take their departures from these borders and especially from these angles. An exceptional portion of the lavas forced from the interior would naturally find exit along these border tracts of disruption and would aid in building them up with relatively light material. Thus an embryonic framework was established, and naturally became the basis of subsequent growth for the protrusive portion of the earth. It was, of course, at all subsequent times, peculiarly subject to denudation, disruption, depression, coalescences, and other forms of obscuration, but yet, in its mutilated forms, it should be traceable in the salient configurations of the earth even in its adult form.

The disruptions on the borders of the segments would tend to shift the accumulating waters toward their interiors which, if we may trust theory and the example of the basaltic columns, mud-cracks, and like phenomena, should have been relatively sunken at the outset. By interpretation, the primary delineation of the oceans was thus instituted. The outlines thus determined would, in their turn, be subject to modifications as they grew.

In both cases, the modifications should have been many and profound and they cannot here be more than alluded to summarily. The atmosphere had the first handling of all the incoming material, as already remarked. The hydrosphere took a second hand. The shrinkage diastrophism sprang chiefly from the accessions thus controlled. It should not be a source of surprise if these three powerful agencies shall be found to have wrought not a few profound distortions in the infantile framework, to have built out not a few apophyses marked by peculiarities of their own, and to have weakened or destroyed some sections of the primitive framework. Coalescences of parts of the ideal structure will naturally have obscured other features. Even if space permitted, it would not befit our theme—which is genesis rather than evolution—here to enter upon a delineation of this complex of formative actions; it belongs to the adolescent history of the earth rather than to its genesis. We may properly go only so far as to see the process under way—that much may be regarded as genetic.

MODIFYING ATMOSPHERIC INFLUENCES

Planetesimal accessions could reach the earth only by way of the atmosphere. Their final lodgment depended on its action. Its dominant movements are ascent and descent; its north-south circulation is a minor factor. Essentially half the atmosphere is ascensive and half decensive; this must always have been so in the nature of the case. The ascending currents tended to buoy up the planetesimal dust, while the descending currents tended to bring it down. The turbulence of the decensive air near the surface, however, delayed the actual lodgment of the dust and held some part of the lighter portion in suspension until it drifted into regions of ascensive currents. Descending air is habitually dry and the flotation of dust in it is notably protracted. Such protracted flotation necessarily had a sifting effect, favoring the lodgment of particles of greater weight in proportion to surface, while those of lesser weight to surface were held longer in suspension and more largely carried by the horizontal component of the air currents into neighboring tracts where ascensive currents prevailed. It is inferred, therefore, that some small measure of preponderance of planetesimals and planetesimal dust of the higher specific gravity found lodgment beneath the descending currents. It is not supposed that the sifting action would be more than very partial, or that the resulting difference in specific gravity of the deposit would be more than very slight.

As the main outlines of land and sea are assumed to have been already defined by the embryonic framework imposed by rotational diastrophism, and to have coincided more or less closely with the borders of the primitive segments, the fundamental features of the atmospheric circulation should have been controlled by these outlines and hence have been then as much as now. The areas of dominantly high barometer and of descending air should have centered, as now, over the great basins, in the main. These portions of the growing earth thus came to preponderate in specific gravity, in some slight degree.

The planetesimal material that floated longer and reached areas of ascending currents, where the air had a precipitating tendency, was brought down by rain and snow chiefly. This tended, in some measure, to concentrate the dust of lower specific gravity in the areas of ascending air. We are thus forced to take into account the essential features of the atmospheric circulation, since that seems to have played an important part in the distribution of specific gravities in the growing earth.

The most fundamental feature of the horizontal circulation in the juvenile ages probably had, as now, a twofold aspect, an equatorial belt of easterly winds, flanked on either side by high-latitude zones of westerly winds, the easterly and westerly components being deflections from a meridional circulation forced by high temperature in the torrid zone and low temperature in the frigid zones. There are some suggestive analogies between these interchanges between the torrid and the frigid zones, and the reciprocating equatorial-polar action of the lithosphere, both being products of rotation. Both have transition zones not far from 30° latitude. In the atmosphere, there was convergence and crowding of air currents toward the poles, and divergence and deployment toward the equator, which forced a secondary adjustment of the circulation analogous to the secondary deformative effects of rotation. This secondary atmospheric circulation is now tripartite, and probably always was so, because a threefold division best accommodated the currents to the peculiar spatial requirements of a hemisphere. The tripartite features of hemispherical circulation are not very declared or impressive unless attention is directed to them, since they are masked by the primary features, but yet they are very real and are highly important theoretically and economically. They form bi-zonal cycles, or, to use a looser term that better fits their nature, bi-zonal gyrals. They are analogous to the three reciprocating segments imposed by rotation on the lithosphere, and they are very similarly placed. They embrace the chief "permanent highs," and their western borders are defined, at intervals, in a spectacular way, by the tracks of the greatest of the tropical hurricanes. Obscurely defined belts of currents swing about the permanent highs and form the most important surface element in the atmospheric interchange between high and low latitudes. Five of such gyral systems are fairly well characterized, one in the North Pacific, on which Eastern Asia depends for much of its fertility, one in the South Pacific, that gives prosperity to Eastern Australia, one in the Indian Ocean north of the equator, affecting Southern Asia, and one also south of the equator, affecting East Africa, and one in the North Atlantic Ocean, on which the prosperity of the eastern half of North America largely depends. The South Atlantic develops its appropriate area of high pressure and its circulatory loop, but not the typhoon phenomena. These great gyrals are believed to be fundamental features of terrestrial circulation, due more to the inherent dynamics of circulation than to the configurations of land and sea, though the two agencies are co-operative and the physiographic configurations perhaps locate the gyrals. If fundamental features, they doubtless had their place in the circulation from the outset. To be sure, they *seem* to be determined now by the great features of land and sea, but they are perhaps only localized by them. If strictly dependent on topographic features, the juvenile reliefs of the lithosphere on the borders of the primary segments were probably sufficient to make them features of the juvenile circulation.

The effect of these bi-zonal systems of circulation is to cut the 30° zone of dominantly descending circulation into three segments and to give the belt a beaded form well shown on modern meteorological maps, especially those of the Southern Hemisphere, where symmetry prevails and the deployment is most nearly normal. The superior accession of planetesimal dust of high specific gravity due to descending currents was thus measurably bunched in the hearts of these bi-zonal gyrals—in other words, in the areas of high atmospheric pressure over the oceans.

MODIFYING HYDROSPHERIC INFLUENCES

The broad features of the juvenile hydrosphere must have been determined by the reliefs of the lithosphere, the embryonic framework, and this, by hypothesis, had been given its basal features by rotation. The circulation of the atmosphere probably in all ages, as now, gave direction to the ocean currents. Whatever planetesimal dust fell into the oceans no doubt floated even longer than in the air and was more effectively distributed over the areas embraced within the ocean circulation. The evolution of the oceans doubtless always tended toward circularity of outline; their primary effect on the dust delivered to them by the air was an increased circularity in its distribution. In so far as the atmosphere was predisposed to deposit its dust in belts, the oceans measurably thwarted this tendency and gave the dust a more circular distribution. We have, however, just observed that the atmosphere, though primarily belted, is measurably suborganized into three cyclical divisions, centered now—and probably always—over the oceans. The notable concentrations of descending air, at present, lie over the three oceanic sections of the 30° belt of descending air. To somewhat similar concentrations in juvenile times, the first step toward the concentration of the denser material is assigned; the ocean circulation, to which this was next committed, advanced the work a step farther by its cyclic action. In this combination lies an assignable first reason for the higher specific gravity of the sub-oceanic segments, an important fact now well established by geodetic and other evidence. In Figs. 30–38 we have introduced ovals within the six segments to emphasize the element of circularity interpreted as having been imposed by the atmospheric and hydrospheric agencies on the original quadrilateral outline assigned to rotational stresses.

To these primitive agencies that took part in localizing the denser and the lighter planetesimals, respectively, there were added secondary agencies that further effected, in a mild but systematic way, the distribution of specific gravity in the growing earth.

SECULAR PERPETUATION OF DIFFERENCES OF SPECIFIC GRAVITY

During all the stages of growth, the planetesimals that fell into the waters were measurably preserved from decomposition, while those that fell on the land suffered weathering and wash. The dissolved portions were added to the oceans, and either remained in solution in their waters or were deposited on their bottoms. The residual matter was left as a lodgment product on the land or, more largely, was laid down close about the land as sub-sea terraces. In the final state reached by these several portions after deep burial and metamorphism, the residuum left on or about the land apparently came to have somewhat less specific gravity than the part added to the ocean segments. The secular processes seem thus to have tended to perpetuate the superior density of the sub-oceanic segments and the greater levity of the continental segments inherited from the previous processes. At the same time, matter was constantly being added to the basins at the expense of the land. It is hence inferred that the sub-oceanic segments were habitually urged to sink, while the continents were forced to rise to restore the equilibrium. This constitutes an enduring, though not an indefinitely enduring, basis for isostatic action, because the actuating differentiation is deeply inbred in the formation of the earth.

During all the ages of growth, the winds swept portions of the light dust of the surface from windward to leeward, and thus shifted material of low specific gravity in given directions and modified the growth of the lands as well as the distribution of specific gravity. The streams and the ocean currents aided even more effectively in giving direction to the growth of the lands and in modifying the configuration inherited from rotational accommodation, while they incidentally affected the specific gravities. So, also, the diastrophism that sprang from shrinkage and other sources cast in its contributions periodically, while vulcanism added its more or less adventitious work. Probably both shrinkage deformations and vulcanism were much influenced by the structural effects and deformative alignments handed down from previous rotational action, but, nevertheless, they doubtless lent their own influences toward shaping the surface features. The final physiographic configurations now presented for study are thus to be interpreted as the joint product of a complex of agencies working together through the whole eon that spanned the growth of the earth from the modest dimensions of its infancy to the full measure of its maturity. In our analysis, the rotational factor is held to have contributed the embryonic framework on which the other shaping agencies built their systematic and their adventitious growths, each in its own fashion.

BASAL FEATURES OF THE GREATER CONFIGURATIONS

To justify the foregoing deductions, the embryonic framework of the earth should be traceable, even at the present time, in its master-features at least, however much they may be disguised by the effects later superposed by other agencies, for fundamental elements bearing such distinctive characteristics could scarcely be wholly obliterated by subsequent events or be the products of accident. Besides, the disguising factors should bear their own characteristics, and these should show some relations to the basal factors. It will aid in tracing the embryonic elements if the leading features that mask them are first pointed out.

It seems clear that the southern half of the earth-body is formed of heavier material than the northern half, for this is implied by the greater mass of water drawn into the Southern Hemisphere and the greater depressions that have taken place there, these two results being the joint effects of a common cause. This dominance is naturally expressed in fewer and simpler yield-tracts. The embryonic lines should there be least distorted. But a broad trough, apparently arising from an intervening cause, lies between 35° and 65° S. Lat., encircling the globe and holding the southern seas. This is to be regarded as having depressed and disguised the three radial yield-belts that, in the ideal scheme, should have diverged from the South Pole in response to rotational stresses.

The Northern Hemisphere is clearly the yield-partner of the Southern Hemisphere; as such, it has been squeezed up, while its partner was depressed; it is hence more distorted and stands forth above the sea-level more notably. The crust is here more broken and more diversified by folds, faults, and other striking forms of relief. The zone between 40° and 70° N. Lat. is about as markedly protrusive as that between 35° and 65° S. Lat. is markedly depressed. In harmony with these contrasts, the radial yield-lines of the north polar region show notable distortion and deflection. As to the cause of these contrasted northern and southern features, tentative views are entertained, but they are too immature and too much aside from our main theme to find a place here. While these causes were probably inherited, in a germinal sense, from the primitive agencies, their main effects seem to be referable rather to later than to earlier stages of deformation and hence they are superposed and tend to obscure the fundamental features.

It is logical to look rather to the Southern than to the Northern Hemisphere for the simplest and least disguised outlines of the primitive framework, for the heavy master-segments are naturally less subject to distortion than the yield-segments. So, in turn, there is reason to look to the Northern Hemisphere for a more marked expression of divergencies and of superadded effects assignable to the work of the co-operating agencies. This is especially true of such agencies as were dependent on protrusion for their efficiency, for example, erosion, transportation, and deposition, which led to outgrowths from the yield-tracts and which also, by loading and unloading, led to deformation.

Lying between these high-latitude belts of the two hemispheres, the equatorial belt should show intermediate proportions of elevation and depression, but here the characteristic lines of the embryonic framework are oblique, not meridional, and are so fundamental that they should show through all disguises and constitute a decisive criterion.

A source of superficial disguise by outgrowths and displacements—springing probably also from the preponderant specific gravity of the Southern Hemisphere—is the lower temperature, and hence dominant force, of the atmosphere of the Southern Hemisphere, probably the result of the larger water surface and the scantier lands of that hemisphere. If thus caused, the preponderance probably extended far backward and conditioned at least the later growth of the earth. As a result of this, the thermal equator lies, and probably has lain throughout the geologic ages, north of the rotational equator, so that the ancestral circulation of the Southern Hemisphere was, as now, relatively free, full, and systematic, while that of the Northern Hemisphere was cramped and distorted. The out-building from the embryonic framework in the Southern Hemisphere was therefore relatively free from idiosyncrasies; that in the Northern Hemisphere much more peculiar and divergent. Leeward out-building might well have been pronounced where the primitive lands were large; the framework might well show peculiar eastward-trending apophyses in the middle and high latitudes. In the equatorial belt, the outgrowths might naturally appear as westward accessions; the broad westward-facing noses of Africa, South America, and Australia are perhaps the cumulative effects of the recurving circulation of the three great atmospheric gyrals referred to in a preceding paragraph. They are among the most singular apophyses anywhere attached to the embryonic framework. The interpretation of the details of the outgrowth from the framework is quite as fascinating as the tracing out of the more basal lines, but it belongs rather to later geology than to genesis.

The protrusive effects of rotational stresses should have been mainly felt at the angles where the yield-tracts joined one another; subordinately along the yield-tracts themselves. The continents should therefore have grown up from these angles as centers, and have been guided by the yield-tracts, and by the co-operating agencies in their extensions. In the Southern Hemisphere, it will be seen that the growth was mainly northward, or toward the yield-hemisphere in the dominant direction of wind and current movements, and that it took place mainly between the bifurcating lines of the yield-tracts (see in particular Figs. 32 and 33). In the Northern Hemisphere, the growth will be seen to have been mainly northward also, and notably eastward, or leeward, in the higher latitudes.*

In addition to these obscuring features of continental development, we have yet to take into account the diastrophism that sprang from secular loading and unloading, and from the shrinkage of the earth-body which appears to have assumed the leadership in shaping the earth after growth ceased, but let this rest for the present. Let us turn now to the tracing of the embryonic framework beneath these obscuring features:

1. Starting with the dominant hemisphere, there should be three yield-tracts diverging at wide angles from the South Pole, all directed northward toward the fulcrum zone. When this is reached, the chief yield-tracts should fork, or at least turn at a pronounced angle, and *strike obliquely across the equatorial zone* to the fulcrum zone of the Northern Hemisphere, where the meridional direction should be resumed and the three yield-tracts converge toward the North Pole. These are singular features and are held to be critical and decisive.

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* At first thought geologists will be disposed to challenge this because post-Cambrian growth has often been in a different direction, but it is to be noted that the later growth was determined by the persistent protrusion of the regions of least specific gravity, and these were likely to be those that had grown most by eolian and aqueous action in the earlier stages.

The lines from the South Pole to the zone of bifurcation or angulation, not far from 30°, are fairly realized in the oft-cited southward-pointing extremities of Africa, Australia, and South America (see Fig. 30). These all suffer, to be sure, from the circumpolar sag in which the southern seas lie, but all of them are connected with Antarctica by sub-sea features, or at least suggestions of

FIG. 30.—South polar view of the globe showing the relations of the southern points of South America, Africa, and Australia to Antarctica and the South Pole, and the tripartite division of the south polar region. The radiant lines merely suggest the approximate positions of the yield-tracts, which of course take on natural flexures. The ovals suggest the somewhat circular configuration of the oceans.

such features, and there is much biologic evidence that there were more ample connections in the early geologic ages, late as these were in the bodily growth of the earth.

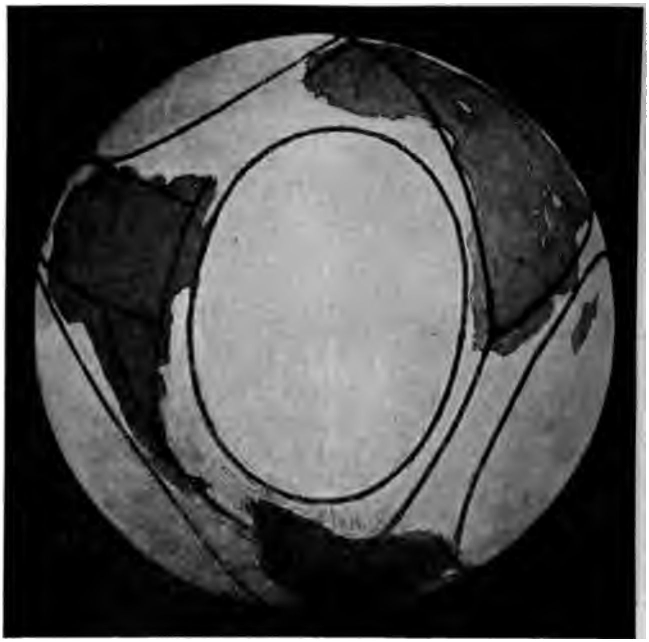

FIG. 31.—South Atlantic view of the globe, showing also the South American and South African bifurcations and angulations of the main structure lines and yield-tracts, indicated by the straight lines which outline the quadrilaterals inclosing the oceans.

2. As the yield-tract directed toward South America reached the fulcrum zone, or its vicinity, its angulation and bifurcation are strikingly realized (Fig. 31). The main continental trend turns sharply to the northwest,

while there branches to the northeast a less dominant but important structural tract, the "Backbone of Brazil." The main trend to the northwest holds strongly—overlooking the rounded westward-facing outgrowth—and is extended in good alignment through the Isthmus of Panama, the Central American States, the Antilles, and onward into the United States to the critical zone appropriate for a reversed angulation into a meridional trend (Fig. 32). This reversal is fairly

FIG. 32.—Antillean view of the globe, showing the northwest-southeast trend lines and their angulations with the meridional trends in the high latitudes of both hemispheres.

well expressed in structural trends which hold for a while, but they soon become obscured by deflections and divergencies referable to special continental deployment.

From the point of first angulation in South America the structural belt trending to the northeast, embracing the "Backbone of Brazil," suffers a break where the two Atlantics coalesce, but it may be regarded as having a recovery and a continuation in the structure lines that skirt the northwestern border of Africa. The southwestern deflection of the Atlas range, though a late feature, was perhaps guided by the old yield-lines.

3. The axis diverging from the South Pole toward Africa seems to have suffered most from the sag under the south seas, but on entering South Africa it bifurcates in about the appropriate latitude, and two ancient crystalline terranes strike northeasterly and northwesterly, respectively, nearly parallel to the two flanks of the African continent (Figs. 31 and 33). The mutual divergence of these tracts falls short of the ideal angle, suggesting an appression from the sides. This suggestion is in harmony with the configuration assigned the whole quadrilateral and with the peculiarities of the European segment. It is also in harmony with the notable protrusion of Africa which, throughout geologic history, so far as known, seems to have been the most uniformly protrusive of all the continents.

At about the appropriate latitude in the Northern Hemisphere, the northwest-trending branch of the African yield-tract is interpreted as angulating to the northward and following the general direction of the west border of Europe. The northeastward-trending yield-tract is interpreted as angulating in the opposite direction, and striking northward along the Ural axis toward the North Pole.

4. The third yield-belt diverging from the South Pole seems to have developed two subtracts of easement, the one striking toward Australia, the other toward New Zealand (Fig. 34). On reaching the fulcrum zone, both of these give place to a pronounced group of northwest-trending axes striking through the East Indies and into

FIG. 33.—View of the Eurafrican quadrilateral, showing its somewhat appressed form, its angulations, and the Caspio-Mediterranean cluster of depressions grouped by an oval after the method used to indicate the circularity of the oceans.

Asia, where, at the appropriate latitude, they reach a most remarkable center of new trends which, while northward, are affected by a pronounced deflection to the northeast. The northwest-trending yield-tract through the East Indies is very pronounced and highly complex, but a northeastern belt is not developed. The submerged configuration of the half-developed Australasian continent very closely resembles that of

FIG. 34.—An East Indian view of the globe, showing the dominant trend lines about it and their angular relations to the meridional lines of the higher latitudes, north and south, as well as their relations to the adjacent great basins.

South America. The yield-belt that, in an ideal scheme, should have connected Australasia with North America is replaced by an apparent coalescence—or non-severance—of the North Pacific and South Pacific basins, more likely their non-severance than their coalescence after an earlier severance. This non-severance is analogous to the less complete effect already observed between the North and South Atlantic. The suggestion is that where two quadrilateral segments in opposite hemispheres were so related as to work together in some degree in responding to rotational changes, it was possible for them to remain united on one flank provided the other moved quite freely by way of compensation. Such free movement of the west flank seems to have been quite fully realized in the unusual flexibility of the East India tract standing over against the united east borders of the North and South Pacific (Fig. 34), and in the similar flexibility of the West Indies in compensation for the partial union of the North and South Atlantic on the east side (Fig. 35). If this interpretation is valid, it is not strange that the joint Pacific segment, by far the greatest of all, should have required for compensation on its western side so marked and so broad a tract as that embraced between the two lines drawn on the illustrative globe, which are extended to each pole.

North of the North Pacific segment there are notable deviations from our ideal scheme. The most notable of these is the Alaska-Siberian bridge between the American and Asiatic continents, which cuts off the apex of the North Pacific quadrilateral. The present expression of this is late in origin. If the initiation of the bridge is not really late in origin, it has certainly been strongly accentuated in relatively recent geologic times, and so it is perhaps more largely referable to shrinkage diastrophism than to embryonic causes. We will refer to this later.

FIG. 35.—North Atlantic view of the globe, showing the relations of the coalescent ocean basins to the yield-tracts lying west of them, particularly those of the West Indies.

While this and other divergencies, shortcomings, and overplacements are not to be ignored, the prevailing obliquity in the equatorial belt and the prevailing meridional trend in the higher latitudes—especially in the dominant hemisphere—are so pronounced that they can scarcely be without fundamental significance. Some of the incompleteness of expression of the basal framework is perhaps referable to a phase of the very principle on which the whole segmentation is based, viz.: the tendency of rigid bodies to concentrate easement movements in zones of freest yield and to reduce disruptive lines elsewhere to the minimum. There seem to be two notable expressions of this:

1. Diastrophic movements in the equatorial belt seem to have been concentrated, to an exceptional degree, in the oblique yield-tracts so conspicuous in the East Indies and in the West Indies, both of which habitually suffer seismic and volcanic disturbances even to this day. Unusual flexibility in these tracts, on the west flanks of the great paired oceans, stands over against the coalescence of the North and South Pacific, and the North and South Atlantic on the opposite sides of their respective segments, as already noted; and so, as a result, the six segments of the ideal scheme coalesced partially into three working pairs, and so the ultimate working segmentation became about as nearly trifid as hexafid, a marked simplification.

2. During the latter half of the adult ages of the earth, there seems to have been a tendency to concentrate diastrophism in two great deformative tracts so crossing one another as to relieve, in large part, the greater stresses that arose in the earth-body—a tendency to reduce tripartition to bipartition, as the earth grew old and stiff. These two great belts, in the latest geologic ages, were "the great world-ridge"—the American Cordilleras projected across Asia and into Africa—and the great Alps-Himalaya mountain tract. Even in the

development of these, however, there appears a marked tendency to take advantage of the yield-tracts previously defined by the basal segmentation. The framework lines were followed by "the world-ridge" through South America, Central America, and half through North America; there was then a "cut-off" to the Asian tract, but in the less pronounced extension across Arabia and Africa, the basal yield-tract was again approximately followed. The transverse Alpine-Himalayan tract also followed the basal yield-tracts in Australia and East Asia, but in its westward extension across the Eurafrican segment it followed the fulcrum zone. In other regions this zone—the junction line between the reciprocating triangular segments, the fundamental units of the whole scheme—shows signs of susceptibility to disruption, but that cannot be dwelt upon here. It is merely possible to emphasize briefly the fundamental tendency toward simplification, as is appropriate in a rigid globe, growing more and more rigid, no doubt, as age creeps on.

But perhaps the most singular and significant of all such features of the earth's surface is the alternate, or offset, positions of the northern factors relative to the southern, as the offset to the northwest of the North Atlantic relative to the South Atlantic, of North America relative to South America, of the North Pacific relative to the South Pacific, and of Asia relative to Australasia. The offset of Europe and North Africa relative to South Africa is much less pronounced, and falls in with the general suggestion of appression already noted. This prevailing alternation of position is closely in accord with its assigned origin in the dovetail arrangement of the working segmental pairs that constituted the assigned primary segmentation.

EVOLUTION OF THE SUB-OCEANIC CONES

Although it is natural to give first attention to the protruding elements of the earth's anatomy, as we have done, they are not its dominant features; they are really its weak features, its yield-effects. The master-features are the denser, heavier, stiffer, depressed centers of the segments themselves. Because the borders were disrupted and protruded, and came in consequence to be the tracts of lesser specific gravity, as already set forth, the waters gathered progressively toward the centers of the segments. If it were safe to follow strictly the analogy of the basaltic segments, we might assume that the borders, and particularly the angles, of the original six quadrilateral segments were elevated while the centers were depressed. Probably this was so, but without trusting too much to this, the dynamics of the case led to central depression as growth went on—and to some interesting migrations of these depressions as well. We have assigned reasons for the concentration of the heavier material in the centers of the oceanic basins. As the oceanic waters gathered, they lent their weight to the further depression of the abysmal basins. Though more or less angular in coastal details—the effects of minor agencies—these basins appear to have grown in general circularity as the natural result of the erosions and depositions of the gyrating oceanic currents. In the accompanying figures ovals have been drawn to emphasize this general circularity (Figs. 30–38).

By the progressive concentration of the heavier material in them and upon them, in the ways already set forth, the sub-oceanic segments were subjected to greater vertical pressure than the land segments at all times during the long era of their growth and even subsequent to their appreciable growth. There was periodic yielding to this in the interest of equilibrium of stress, and this involved lateral pressure in addition to

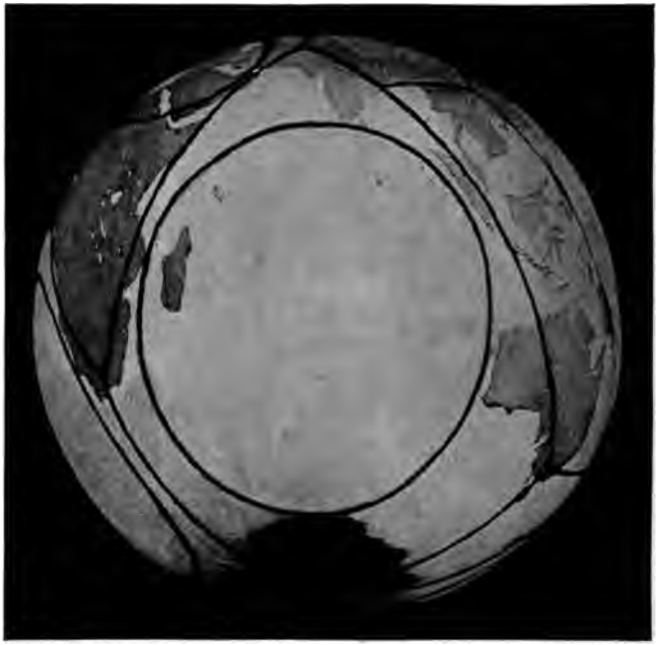

FIG. 36.—View of the Indian Ocean, showing its fundamental circularity, shown by the oval, and the main yield-tracts adjacent to it, indicated by the straight lines which define the quadrilateral within which the ocean lies.

the vertical pressure. Under the law that equal pressure in all directions increases rigidity, the sub-oceanic segments should have acquired degrees of rigidity superior to those attained by the continental segments, which, as the yield-segments, suffered more from differential stresses. The sub-oceanic segments enjoyed some advantages of attitude and, on account of this, suffered less surface deformation, and probably less internal distortion also, and for these reasons they should have been less affected by schistosity and the allied adaptations to easement movements.

In so far as the sub-oceanic segments came to have rounded outlines at the surface, their downward extensions would come to be conical rather than pyramidal. To this extent, it is fitting to speak of them as cones. Whether they are to be regarded as frustums of cones, reaching merely to the horizon at which planetesimal growth began, or as complete cones, developed to the earth's center by superior pressure and by dominance in diastrophic action, we need not turn aside here to consider. Whether frustums or completed cones, they became the master-factors in the shaping of the earth by reason of their superior specific gravities, their superior rigidities, their relative freedom from yielding tracts, as well as their progressive loading which served constantly to keep them under growing pressure.

Five of the ideal six oceanic basins attained pronounced development, the North Atlantic, the South Atlantic, the Indian, the North Pacific, and the South Pacific. Under these let us picture five great, heavy, stiff, sub-oceanic cones. What should have been the sixth, in an ideal development, is represented by the broken, pitted Caspian-Mediterranean group of basins, with its strange assemblage of abrupt depressions and elevations, not to speak of the strangely elongate fossae in which lie the Red Sea, the Dead Sea, and the Adriatic (see Fig. 33). This remarkable combination may perhaps be regarded as a substitute for the ocean that ideally should have had a place in the heart of the Eurafrican quadrilateral. This abortive result seems to find a correlative in the half-developed Australasian continent on the opposite side of the globe. In the earlier geologic ages the quasi-oceanic mediterranean cluster of basins embraced wider and deeper depressions than now, but it does not appear ever to have been merged in a continuous abysmal basin, at least not in known geologic history.

The basins destined to become truly abysmal were probably unequal in area and depth at the start, for inequalities would almost inevitably arise in the primary segmentation of such a body as the earth yielding under such a complex of stresses as were brought to bear upon it. Such inequalities as arose at the outset, or in the early stages, naturally influenced the adjustments of the segments to one another in the later stages, and so led to progressive encroachments of the greater on the lesser, and these primitive inequalities grew, in time, into the still larger inequalities of today. The results of the struggle between these factors of different powers claim a moment's attention.

A very natural effect seems to have been the pairing of the weak with the strong in antipodal positions. The heavy, relatively rigid, sub-oceanic cones stand opposite the lighter, weaker, yielding continents. The heavy rigid factors came also to be larger than the lighter weaker ones, in about the ratio of two to one, surface measure. The embossment of North America lies opposite the basin of the Indian Ocean; the embossment of Australia lies opposite the basin of the North Atlantic; the great protuberance of Africa is antipodal to the abysmal depths of the Central Pacific; the greater mass of Asia is antipodal to the South Pacific, while

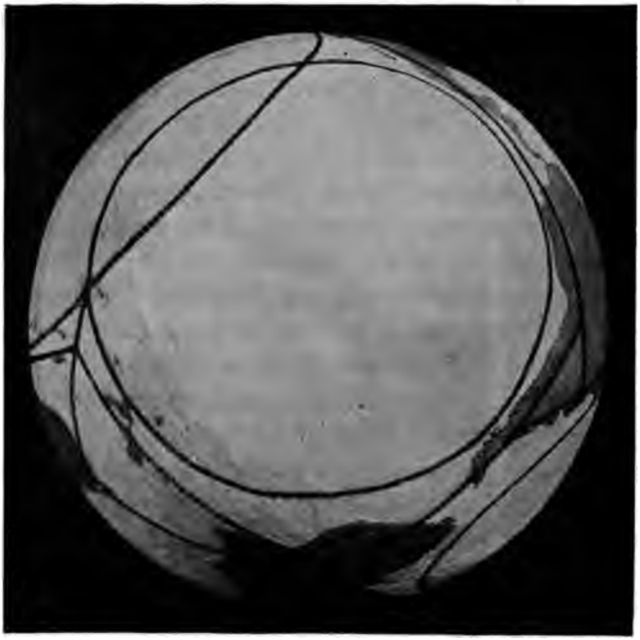

FIG. 37.—South Pacific view of the globe and the adjacent yield-tracts on the east, south, and west, and its coalescence with the North Pacific, indicated by the overlapping of the circles, which define the fundamental circularity of the oceans.

its eastern extremity lies opposite the South Atlantic basin; South America is antipodal to the westward extension of the North Pacific. The equatorial part of the South Atlantic basin is not represented by an antipodal protrusion, though its southern part lies opposite the Alaska-Siberian bridge. Thus the law of opposites, as respects both position and dynamic power, is well sustained, though not perfectly realized. The heavy, rigid, sub-oceanic masses, with their superior tendency to work toward the earth's center, are rather definitely correlated with the lighter, weaker continental masses that have shown a yielding, protuberant tendency throughout geological history.

If all deformative actions are pictured as due to mere local or regional surface pressure acting on a viscous or plastic earth-body, some very debatable questions as to the transmission of stresses to the opposite side of earth naturally arise; but if the rotational, the tidal, and the larger stress-effects of loading and unloading are pictured as stress-differences affecting every part of the body and the central portions in highest degree, and if these stresses are conceived as acting preponderantly through the relatively rigid sub-oceanic cones, the stiffer set of factors, while the weaker set of factors opposed to them are rendered susceptible of yielding by zones of schistosity, the struggle for advantage of position takes on a different aspect; it becomes chiefly a simple contest of mechanical push and yield that reaches its climax in the deep interior. The stiff heavy cones naturally should have found accommodation by pressing into the yielding tracts opposite them. An alternate adjustment with the opposing cones of their own stiff type may be regarded as a necessary incident.

An inevitable result of every stage of progress of the heavy, rigid, sub-oceanic cones in working toward the earth's center was a lateral crowding of the weaker, lighter, continental wedges that filled the space between them. The observed secular tendency to up-yielding, on the part of the continents, as the reciprocal to the observed oceanic tendency to work downward, conjointly with the inevitable lateral thrust, thus appears as a logical consequence of the whole process of growth and adjustment. The struggle is interpreted as but a method of seeking isostatic adjustment in a form not only wholly consistent with the earth's essential rigidity, but actuated and controlled by the superior rigidity of the sub-oceanic cones.

These aggressive actions of the stronger at the expense of the weaker were expressions of *the law of dominance*, while at the same time they came to be illustrations of *the law of opposites*, and of *the law of alternates*. The stiff heavy cones should naturally have grown more and more dominant as their masses and their rigidities increased, and as their competitors suffered in resisting power by yielding and suffered at the same time in weight by erosion at their surfaces.

Naturally enough, a dominance of the greater sub-oceanic cones over the lesser of their own class appears. The three greatest sub-oceanic cones—the great triumvirate that lay beneath the South Pacific, the North Pacific, and the Indian oceans, respectively—not only appear to have mastered and crowded aside their lesser competitors but to have joined forces in pushing toward the earth's center. In so doing they seem not only to have drawn a superior measure of the hydrosphere and of its dissolved rock in upon themselves, resulting in the well-recognized water-hemisphere, but to have forced

FIG. 38.—North Pacific view of the globe, showing the relations of the abysmal basin to North America, Asia, and Australasia, to the Alaska-Siberian bridge, and to the South Pacific.

the weaker cones beneath the two Atlantics, and the continental wedges that clustered about them to yield, and by such yielding to have formed the land hemisphere. A bilateral asymmetry was thus imposed on the earth. Some measure of this might indeed be accomplished by a shift in the center of gravity of the earth, without diastrophism, but the latter seems to us inevitable also.

We have spoken of the Southern Hemisphere as dominant in specific gravity, in subsidence, in water-accumulation, in lower temperature, and in heavier atmosphere; but, more accurately speaking, the center of dominance in most of these respects lies near the junction of the three great oceans; causally it is to be referred to the heavy cones that lie beneath them. The hemisphere of preponderant gravity, and hence of abysmal areas and of water bodies, centers not far from New Zealand, while the compensatory protuberances are distributed about some point in Southeastern Europe, a region of notable instability, of tortuous folding, of pronounced overthrusts, upthrows, and downthrows, and of igneous and seismic phenomena. There may be no causal relationship in this, but it is suggestive. This largest of deformative concepts, that of hemispherical adjustments, compensations, and balancings, involves extremely massive movements, but perhaps they may all be realized by the wedging action of essentially rigid elements separated by schistose tracts; if so, in their combination they would form an essentially rigid earth.

In thus carrying forward into the interpretation of the later physiographic configurations some rather remote deductions drawn from the planetesimal dynamics engaged in the shaping of the juvenile earth, we are painfully conscious of the high probability that we have fallen into some misconceptions, perhaps not a few, and have entertained views that will need to be rectified. But even misconceptions may be suggestive;

they are likely to be more suggestive than no conceptions at all. The strong trend of evidence, converging from several quarters, pointing almost unequivocally to a pervasively rigid earth, lends some degree of sanction to almost any serious attempt to build up a system of dynamic concepts that are consistently loyal to the known behavior of crystallizing rock-masses, as such, and that scrupulously leaves them in the full possession of their typical qualities at all stages of the earth's reshaping.

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CHAPTER IX

INNER REORGANIZATION OF THE JUVENILE EARTH

The heterogeneous way in which the accessions from the parent nebula were gathered into the growing earth, their intermixture with air and water, their partial oxidation, carbonation, and hydration, and their progressive burial deeper and deeper as growth proceeded, were eminently fitted to call into action a series of readjustments, recombinations, and recrystallizations in the interest of a better accommodation to the new conditions that gradually arose in the interior. Not only was each layer pressed by the layers that accumulated above it, but the force of gravity grew with each accession and the growing body pulled itself together with increasing power as time went on. The total shrinkage of the loose surface matter in reaching its final compact form was nearly a third of its original volume. A portion of the energy that had been engaged in maintaining the volume of the mass was turned by compression into heat and this powerful agency for both chemical and physical change was brought into action. At the same time and by the same act, growing pressure was brought to bear, the normal effect of which is increase of rigidity. There thus arose in the interior a contest of co-operative but yet antagonistic agencies, a contest not unlike that waged by the geologic triumvirate on the surface.

THE RADIOACTIVE FACTOR

There was here also a third agency, atomic dissociation. Radioactive elements in undergoing spontaneous decomposition, then as now, no doubt shot streams of electrons and alpha-particles into the adjacent matter at such prodigious velocities that they penetrated to appreciable distances and constantly tended to raise the temperature. Radioactive heat was thus added to the heat of compression. It is assumed that the radioactive elements came in with the other accessions from without and that they were scattered at random through the successive layers added to the earth. Special students of the subject find by computation that, if radioactive matter were scattered through the interior uniformly with a value equal to that in the surface rocks, a layer less than forty miles in depth would generate as much heat as the earth is now giving forth. There is no experimental evidence that such degrees of heat and pressure as prevail in the earth would restrain the disintegrating habits of radioactive matter and explain on this basis the relatively low measure of heat arising from the interior. It is therefore inferred that the radioactive material was originally scattered sparsely through the whole ingathered mass but was concentrated later at the surface. This is quite in harmony with the evidence gathered from fresh effusions of lava presumed to come from considerable depths, from meteorites that come from space, from the sun and the stars, which implies scantiness rather than richness in radioactive elements. It is therefore assumed that there was only a sparse distribution of radioactive elements in the parent nebula, and hence in the original material of the earth, but that there was progressive concentration of these at the surface as effusive igneous action went on.

This revolutionary factor was quite unknown and unanticipated when the planetesimal view of earth-problems was first entertained, and it is interesting as well as gratifying to see how happily it falls in with the processes already postulated. The new factor is to be pictured as giving rise to a multitude of minute self-heating centers scattered at random through the growing mass and adding sharply localized heat to that which arose more uniformly from compression.

STRESS CONTROL OF THE INTERIOR

Let us not fail to note, at the outset, the stress-conditions, for they are held to be a vital agency in forcing liquid matter to the surface as fast as formed in workable quantities, except as its specific gravity may have been high enough to resist this. Besides the simple hydrostatic pressure of gravity, equal in all directions, there were stress-differences arising from changes of rate of rotation, from the pulses of the body tides, and from shrinkage. Let us accept the modern view that equal pressure in all directions tends toward rigidity, while unequal pressures, or stress-differences, favor solution, fusion, recrystallization, and like changes. The gravitative pressures—which now range from one atmosphere at the surface to about three million atmospheres at the earth's center—were indeed less than this while the earth was in its juvenile stages, but proportionally less force was required to expel liquids then. The special nature and the high competency of the rotational stress-differences have already been emphasized. The tidal stress-differences were small and no doubt always fell within the elastic limits of rock except when very close to the yield-point from other causes; none the less, a perpetual alternation of even minute stresses and reliefs, acting on rocks whose temperatures were rising and whose molecules were approaching the critical point of loosening their holds, was quite certain to accelerate such loosening by adding the critical amount of stress needed.

Without doubt, the most important feature of the tidal and the rotational stress-differences in this connection lay in the vital fact, already repeatedly emphasized, that they were greatest at the center and graduated outward. Their action was not unlike a vise closing from below. They were superposed on the hydrostatic stresses of gravity which graded still more strongly from the center outward, and which, though pressing equally in all directions at any given point, yet had a selective effect which of itself tended to force the lighter liquids to the surface. The nature and distribution of the stresses that arose from shrinkage and other sources were too complicated to be discussed in this connection. It may merely be noted that all the lands were suffering loss of weight, at least relatively, and that all the oceans were gaining load. When it is considered that the mechanical sediments were chiefly gathered in shelves about the borders of the lands and that they thus formed a network enveloping the earth, and when it is further considered that the material dissolved from the lands was diffused through the oceans, increasing their weight, and that they enveloped the earth in a belt of broad areas, it can scarcely be questioned that these two comprehensive systems of loading the earth generated widely prevalent and deep stress-differences, and that their general effects on mobile matter within the earth must have been analogous to those of the rotational and tidal stresses.

Taken conjointly, these seem amply to warrant the view that the earth-body was at frequent intervals, if not more or less constantly, permeated by stress-differences whose tendencies were to force to the surface all mobile material whose specific gravities were insufficient to resist them. This was a mechanism well suited to preserve the solidity of the earth against increasing heat and growing liquefaction by forcing the heat-product and a vital portion of the heat itself to the surface.

The precise mode by which lavas find their way through solid continuous rock, such as prevails below the zone of fracture, is a problem not yet fully elucidated. That they do penetrate such rock seems to be placed beyond question by the hundreds of volcanic vents that must apparently connect with the heated regions that lie much below the zone of fracture. The fact that igneous effusions avail themselves of fissures in the zone of fracture does not answer the question of their mode of penetrating the unfissured zone below. The upper part of this zone of continuous rock seems clearly to have a much lower temperature than molten lavas and so has a chilling effect on them which is an adverse factor. So, too, the stresses thus near the surface are generally of the lower order, except at times of diastrophism. These factors make the present-day problem quite as insistent as that of the earlier ages and of the deeper horizons. The process of penetrating the cold crust would seem to be somewhat more difficult than that of lower horizons where the temperatures are higher and where the liquefaction-curve is probably nearer the temperature-curve. As lavas have been poured forth at all known geologic ages and at a multitude of points on the earth's surface, there seems no need to regard the possibility of escape of lavas as a special question affecting our problem. The fact that the earth is now giving forth lavas at many points, at intervals and in little driblets, though the earth is certainly now essentially solid and even highly rigid, is interpreted as a living expression of the view here urged, to wit: that the mechanical stresses within the earth force lavas out essentially as soon as they accumulate to working volumes. The small average size of present eruptions implies that the quantity of lava required to constitute such working volume is small. In harmony with this is the significant fact that no damping effects of molten rocks on the transmission of earthquake waves, nor on the elastic response of the earth-body to tidal stress, have been detected. Pronounced tidal movements might be expected in the necks of volcanoes if they were connected with large reservoirs of lava below, but if there is any response to tidal strains at all, it is scarcely detectable. The independence of volcanoes quite near one another is pointed evidence adverse to a liquid connection and to great subterranean reservoirs. Nor is this mitigated by evidences of sympathy of a certain sort, when under the same stresses, or affected by the same source of agitation.

If we turn for a moment to the constructive side of the problem, the process of rock-flow by recrystallization offers suggestions in the hints it gives of the ability of free matter to move through solid rocks under high pressure. In such recrystallization there is obviously not a little movement of molecules into new positions, else old crystals could not take new shapes and align themselves in harmony with the stresses that force the reorganization; much less could new crystals form. In most of the cases critically studied there does not seem to have been even a close approach to general melting or solution, but only a very partitive molecular action.

In the cases we most wish to consider, the most soluble portion of the mass is assumed to have passed into the liquid state. This, in itself, helped to prepare the way for the movement of this liquid portion by at least partially preparing liquid passageways. If not at once adequate, delay would increase the liquefaction and lead on toward adequacy, so that, long before the whole mass became liquid, a way of escape would be provided by the process itself. It is helpful to observe that, while mean pressures may be the same on liquid as on solid rock, the pressures on the liquid are transmitted equally in all directions and, as a result, the liquid may insinuate itself into the most yielding planes, or into the weakest points, in the crystalline mass; on the other hand, the crystals are subject to unequal strain and to the development of weaknesses between, or within, themselves, of which the liquid may take advantage.

There seems good warrant, then, in observed vulcanism and in theoretical considerations, for the generalization that molten rock is, and probably always has been, persistently squeezed out of the earth by the stresses and strains that permeate it. The periodicity of these stresses and strains gives periodicity to vulcanism, but the limited volume of liquid matter requisite to incite expulsion seems to have extended vulcanism beyond the periods of marked diastrophism because the stress requirements are so small.

With this environment of stress and strain and its expulsory tendency ever in mind, let us turn to the changes which the heterogeneous mixture of material in the interior must probably have undergone as the temperatures, the pressures, and the stress-differences waxed more and more intense with progressive burial. It need hardly be urged that there would be preeminent opportunities as well as insistent demands for physical readjustments, for chemical recombinations, and for adaptive recrystallizations. There would be especial urgency toward those readjustments, combinations, and recrystallizations that brought better adaptation to the increasing heat, pressures, and stress-differences.

DIVERGENT COURSES OF HEAT

The part played by heat was obviously critical. Let it then be carefully noted, at the outset, that each increment of heat was likely to suffer partition into three portions, one to be consumed in forming new heat-absorbing solid compounds, another to produce liquefaction of the most soluble or fusible rock substance, taking the latent form, and the third to remain as sensible heat, maintaining the new temperature status of both rock and liquid. The first remained as energy of organization. The second went with the liquid wherever the stresses and strains forced it, mainly upward and outward. The third followed alternate courses: a part was carried toward the surface with the ascending liquids and later radiated into space; a part was carried toward the surface by conduction; and a part remained as an increment of temperature. This last was always subject to loss by conduction. While there was therefore a tendency to increase the central heat and steepen the temperature gradient, so long as compression remained efficient and so long as radioactive particles remained in effective abundance, there went hand in hand with this tendency to increase a group of checking processes. The probable result was an equally constant tendency toward an equilibrium in which the chief controlling element was the solution or fusion of the most soluble or fusible compound in the heterogeneous mixture. The main action was probably mutual solution rather than melting. It is not known that simple pressure, by the heat it generates, will melt any substance that shrinks on crystallizing—which includes practically all under consideration—so far as experimentation covers the case; pressure, on the contrary, increases the rigidity of all such substances. It was probably, therefore, chiefly the heat of radioactivity, and perhaps certain chemical reactions, that promoted liquefaction. The temperature should then have been regulated by liquefaction and probably never rose above the solution-curve of the most soluble substances that remained at each given horizon. *The thermal gradient and the solution-curve were identical throughout the liquefying horizons.*

SELECTIVE LIQUEFACTION

Now it is important to observe that the assigned rise of pressure was very gradual and the increment of heat arising from it at any stage was very deliberate. So also was the heat generated by the sparse radioactive material. So probably was the heat from all other assignable sources. The portion available for liquefaction at any time, in spite of the restraining influence of pressure, and over and above the part carried away by conduction and liquid extrusion, and the part consumed in recombination and reorganization, is believed to have been relatively small and to have been sufficient only to cause the solution of a very small part of the whole mass. This part, of course, was that which was most soluble under the existing conditions. The selection was much affected by the particular contacts that happened to be available in each particular spot. The nature of the contacts was obviously a vital matter. The principles of eutectics should have found, in the intimate mixture of the accessional material and in the slowly changing conditions of the interior, a supreme field of action.

The heat arising from the sparse content of radioactive particles was limited and was generated gradually. It was also undergoing steady removal. The radioactive molecules themselves had high specific gravities, but each must have heated by its projectiles some billions of adjacent molecules, so that the average specific gravity of the melt or the solution that resulted would differ little from the common average and would respond to the stresses and strains imposed on the common mass in about the normal way, save that the higher temperature which the radioactivity would continue to generate would favor buoyancy. Even in the little viscous mass generated, the part close about the heating particle should gain a superior temperature and so rise to the top and be brought to bear where it could do most to bore its way upward.

One of the first effects of rising temperature on the heterogeneous accessional mass would be the freeing of the atmospheric and hydrospheric elements that had been entrapped with the accessions, or had united with them. These volatile elements should enter the globules of liquid rock as fast as they were formed, should lower their specific gravities, and should increase their tendency to ascend. The same may be said of other gaseous and aqueous matter that had previously entered into combination with the planetesimals and had been retained and buried with them.

It appears then that the volatile constituents, the more soluble or fusible elements and the self-heating particles, should have formed the earliest lavas. These should have carried to the surface not only a portion of the interior heat, *but a portion of the heat generators.* The succeeding liquefactions should have involved the next most soluble or fusible constituents and should similarly have carried out a part of the residue of self-heating matter. And so the selective liquefying action and the progressive removal of the radioactive element should have proceeded in mutual co-operation toward a predetermined end.

SELECTIVE SOLIDIFICATION

On the other hand, the residue should have increased in average refractoriness by this selective action and by the recombinations that had been induced in the interest of stability. At the same time, the hydrostatic pressure at any given point in the depths was undergoing increase not only by the continued accessions to the surface, but by the removal of liquefied material from the given horizon to a superior one, perhaps the surface, where its weight was also brought to bear.

In addition to these increments, the compressibility was quite certainly losing effectiveness under the general law that compressibility declines with compression. In addition to this, the refractory residue was probably less compressible than the previous mixed mass. Dr. Moulton calls attention to the further significant fact that the potential energy set free as the center is approached constantly declines. These several sources of decline in the effectiveness of compressibility lessened the heat developed from that source at any given point by further surface weighting. If the curve of compressibility is asymptotic, as is probable, the increment of heat in the deeper interior would tend to become negligible; it might even be changed to a decrement, for conduction might be able first to equal and then to surpass it.

The selective process, by the removal of the silicates in larger proportion than the metals and metallic alloys, probably tended to concentrate the latter toward the center and to make them ultimately a large part of the residuum. By this, the conductivity would be increased and be able the sooner to overmatch the small increment of heat arising from the vanishing effectiveness of compression.

There seem, therefore, to be excellent grounds for believing that the selective separation of the inner substance of the earth into a liquid ascensive portion and a solid residuum growing more and more rigid was a definitely declining process and that ultimately a nearly static highly rigid state was reached.

Precedence has been given to selective liquefaction and the effective removal surfaceward of the most solvent or fusible elements in the originally mixed aggregate. It was noted that a certain portion of the heat developed should have promoted endothermic reactions and should thus have passed into structural functions and disappeared as heat. It remains to observe more broadly that the changes in heat, in pressure, in stresses and strains, together with the selective removals, should have been highly favorable to progressive recombinations and recrystallizations. Under the influence of periodic stress-differences superposed on graded static pressures, and under the stimulus of rising heat, new combinations and new crystallizations of the solid residue should have followed one another until the best accommodation to the existing conditions at each horizon was reached.

The inner reorganization of the juvenile earth is, therefore, pictured as a process that affected pervasively the whole interior of the earth, preserving effectively the solid state of the main mass and progressively increasing its average rigidity, while, at the same time, it set free and forced toward the surface, stage by stage, the lighter and more mobile material.

It was previously noted that the inelastic and magnetic material had probably been somewhat concentrated in the heart of the earth, but only very partially so. The processes just described should have pushed the concentration to much greater lengths by the removal of the lighter elements, so far as soluble under the conditions developed, and by differential separation in the liquid state, so far as that obtained. No complete differentiation is postulated even as the ultimate result. The mixed states of meteorites, our best guide in the matter, do not encourage the notion of complete segregation.

LIQUEFACTIVE CONTROL OF TEMPERATURE

If the preceding sketch of inner reorganization is in the line of truth, the interior heat should never have risen to the great heights that have often been assigned it, for the heat was progressively consumed or carried away. The process was long, for the growth was gradual; much of the material came to the surface, again underwent burial, reheating, and reorganization; again came to the surface, and so on; the heat removal was proportionate. The thermal curve of the interior should have been determined at all stages automatically by the contesting agencies, and should have been coincident with the curve of selective solubility for that stage. A completely liquid state—much less a central gaseous state—are held to be incompatible with the mechanism that presided over the building of the earth. They seem to be equally inconsistent with the testimony of seismic waves and with the tidal responses to the moon and the sun. The preceding sketch harmonizes with this testimony in assigning to the main body of the earth, if not to its whole interior mass, an elastic rigid nature.

The function of igneous effusions in the economy of the earth may be likened to the function of perspiration in the economy of animals, a means of carrying forth to the surface and discharging the excess of heat and of fluids arising from the inner activities of the organism.

The mutations of the inner earth may be summarized as a single prolonged process by which the more fluent, solvent, and lighter material of the earth-body was concentrated toward the surface, while the more immobile, refractory, and heavier matter was concentrated toward the center. The result may be pictured as a central core dominated by metallic alloys and a thick enveloping sphere dominated by silicates. The whole is regarded as essentially crystalline, the crystallization in the depths being controlled by pressure in the interest of spatial economy. In the movement tracts parallel recrystallization prevailed and a schistose structure followed. The arrangement of atoms and molecules in the heart of the earth was probably controlled in the main in the interest of the internal fixation of energy and was predominantly endothermic. In contrast to this, the matter that rose to the surface promoted the dispersion of energy. It is probable that the concentration of the metallic elements toward the center contributed to increasing complexity in the alloys of the earth's core. The interpretation of the deep-seated deformative movements and of the transmission of seismic waves through the central region should probably be guided by the characteristics of alloys rather than of silicates.

CHAPTER X

HIGHER ORGANIZATION IN THE GREAT CONTACT HORIZONS

The contact surfaces between earth, air, and water are the sites of the most distinctive geologic activities of the present day, and as far back as a good record goes these contact zones have been the seats of the most declared denudations and depositions. They have been almost the sole habitats of biological and psychological activity. The initiation of terrestrial life and the dawn of terrestrial mentality within these horizons were the last radical events in the genesis of the earth. As sequences of the nebulous stages, and of the long chain of organizing processes we have tried to sketch, these supreme evolutions take on the aspect not only of extraordinary physico-chemical syntheses, but of syntheses with psychic activities whose fundamental nature has not yet been fully compassed by determinate science. To any who may prefer to regard the vital and mental elements as supernatural additions to the physico-chemical factors, the combinations must still seem remarkable syntheses, for the relationships are extremely close and the interdependencies singularly complete. From the naturalistic point of view, these climacteric developments embody three great steps: (1) an ascent in the complexity of physico-chemical combination until it attained the organic type, (2) an evolution of physiological processes and of organs subservient to these, and (3) the initiation and the varied deployment of psychological phenomena. These seem to have followed one another in ascensive order. Their close sequences, in a common habitat, seem to imply that each earlier step was prerequisite to each later advance, and that the three steps form a single genetic series; but it is perhaps premature to affirm this, for the connecting links, as yet, lack complete demonstration.

QUASI-ORGANIC SYNTHESES

The first step embraces a long chain of complex physico-chemical combinations that do not appear to have been completed either above or below these contact surfaces, or in the heart of the earth, or in any known region, previous to the earth's adolescent organization, but of course it cannot be said that they were previously absent everywhere.

The type of synthesis was notable in being selective rather than general. The outer surface of the earth-body is not very notably synthetic, on the whole, either chemically or physically. Technically, it is the zone of *kata*morphism, the zone of *downward* changes. Igneous effusions from within the earth commonly pass by weathering into simpler combinations. The silicates that form by far the larger part of the outer terranes of the earth-body are rather notably prone to disintegration in contact with air and water and to take on less complex forms. Residual earths, in particular, are rather marked simplifications of the silicate compounds from which they were derived.

Yet, in marked contrast to this katamorphic tendency, there arose a special series of synthetic actions that led the way to types of organization so far transcendent as to require recognition as distinct phases of earth-genesis, indeed, as its most remarkable phases. The series emerged very gradually, it would appear, from the more common activities that had previously prevailed. The action centered about combinations of carbon with the constituents of the atmosphere and of the hydrosphere to which were added special limited selections from the rock constituents. Solar and other energies entered into the synthesis and were perhaps its most vital element. The products are conveniently called carbon compounds. It is the accepted working hypothesis of most students of the subject that there was a continuous series of carbon compounds leading up from such as had long been formed under celestial conditions and in the parent nebula and later in the interior of the earth, and in the air, and in the waters, until, at length, they reached those extremely complex forms that constitute the bodies of living beings. Hypothetically, the whole wide gap between the simpler carbon compounds and the most intricate organic compounds was thus bridged. But, as yet, both observation and laboratory research fall short of fully substantiating this, though encouraging advances in building up such a connecting series are being continually made.

However pronounced, therefore, our theoretical prepossessions may be, and however confidently we may rest on the doctrine of continuity and derivation, it must be frankly confessed that the complete graded series of postulated synthetic compounds is neither observable in nature, nor, as yet, producible by art. Neither the carbon compounds that appear to have arisen in the long past without the concurrence of life, nor those that can now be induced by manipulative skill, entirely fulfil evolutionary expectations. And yet the series that partially bridges the gap is great enough and significant enough to require pointed recognition and sharp emphasis as one of the most suggestive factors in the earth's evolution.

The reason for the present imperfection of this theoretical series lies, perhaps, in the destructive efficiency of the lower orders of life. Ever since the minute forms of life filled the earth with their multitude, the slow graded steps by which alone the long chain of syntheses could be accomplished have been subject to formidable predacious attacks. The ubiquity, the voraciousness, and the digestive efficiency of the multitude of minute pioneers in the living kingdom may well be supposed not only to have destroyed the relics of the primitive series but to have broken down all subsequent attempts before the long ascensive chain could be completed. The universality, the persistency, and the effectiveness of the attacks of bacteria and their kin upon highly complex carbon compounds, while yet in their formative states, seem adequate to inhibit entirely a process that might have been successful before their advent. There is fossil evidence of the presence and of the destructive work of bacteria far back in the geologic ages. There is inferential evidence of their presence and destructive work as far back as the stratigraphic record of life reaches.

APPEARANCE OF LIVING ORGANISMS

Nothing in the whole genetic history of the earth was more distinctive than the first appearance of living organisms. Nothing is more remarkable than the persistency with which these organisms have since occupied the surface of the land, the bottom of the air, the surface of the waters, and the bottoms of the waters. They wander indeed somewhat from the immediate contact surfaces, but none the less they adhere rather faithfully to them or to their vicinity. If scrutinized more closely, it is seen that the critical conditions for the living world are best satisfied when the three factors, earth, air, and water, unite their good offices with those of radiant energy, which appears to be quite as vital a factor in the combination as either of the material factors. We are perhaps inclined to regard radiant energy as even more indispensable than the others, but nothing is more inhibitive of life than certain intensities of radiant energy. It is a remarkable combination, conditioned by singularly narrow limitations.

From our personal and racial point of view, the transcendent nature of the initiation of life needs no insistence. Our personal and racial point of view may not be without its element of partiality, but, considered strictly as a problem of research, the introduction of processes so different from those of the cosmogonic ages as are cell construction, co-operative physiological action, adaptation to ends, the propagation of kind, and the perpetuation of species, constitutes perhaps the most puzzling aspect of terrestrial genesis, unless it be the one that is to follow. There is, in these processes, a subtle factor not distinctly betrayed in the earlier modes of evolution, though it may have been there. It is hard to find any distinct trace of an organizing, correlating, directive factor, as such, in even the most intricate of the inorganic syntheses, and yet there are subtle intimations of something of the kind in the complexity of even inorganic combinations.

It is not clear from any positive knowledge at present available that there was present in the initial stages of life-evolution any sentient or psychological element, and yet cell construction, with its apparent adaptation to ends not necessarily confined to its own immediate economy, co-operative physiological action, with its relations to the life of members not immediately involved in the action, provision for the propagation of kind, with transmitted powers of reproduction, and precautions for the perpetuation of the species, squint sharply in the direction of psychological endowments.

ADVENT OF THE PSYCHOLOGICAL

However occultly the psychological element may have crept into the advancing life-series, its distinct appearance marked the climax of the earth's evolution. It is the last factor in the earth's genesis, and the most enigmatical. Considered with respect to its own inherent qualities, the psychological factor seems to stand apart from the physiological farther even than does the physiological from the inorganic. The bridging of the gap is less explicable, scientifically and philosophically. The distinctive qualities of the psychological world are not commonly recognized in scientific interpretations of the physiological or of the inorganic worlds; but the gradation from the seemingly insentient and merely physiological, as embodied in the lowest orders of living beings, to the earliest types of sentient life, and thence on to the highest manifestations in the thinking world, is so intimate as to bind the whole into one inextricable problem. If the qualities of the psychological world were potential in the factors that entered into the earlier stages of the earth's genesis, a revision of our conception of those factors is logically required. Fundamentally, the antecedents can scarcely be less comprehensive than the consequents. If the material begets the spiritual, the material can hardly be altogether material. If the mechanistic indulges in research, it cannot well be altogether mechanistic.

Our province, however, lies rather with the geologic conditions that attended the origin of terrestrial life and of psychic action, than with their ultimate nature and their fundamental relations.

EARLY BIOGENETIC CONDITIONS

Under the planetesimal hypothesis, the progressive work of uniting solar and other energies with carbon, hydrogen, oxygen, nitrogen, sulphur, phosphorus, potash, and other elements, into a synthetic chain which led up to biotic and psychic organisms, found suitable conditions at an early stage of the earth's juvenile history. As already observed, the ascending synthetic series was free from the adverse effects of its own development, free from predaceous attacks by minute organisms, now a formidable obstacle to such an evolution. Synthetic combinations were free to go to the utmost lengths to which inherent organizing forces impelled, except as restrained by inorganic limitations. The essential conditions of heat, light, air, water, and earth surface are not pictured as then radically different from those of the later geologic ages and of the present, though not altogether identical with them.

The sun was indeed younger in those ages and, under current views of stellar evolution, more intensely radiant, but the nebulous material that intervened between the sun and the young earth should have cut off some part of the solar radiance. However, there should have been compensation in the heat and light generated by the plunge of planetesimals into the upper atmosphere. The ratio of such compensation is uncertain; it is merely evident that the planetesimal interferences and the planetesimal compensations followed the same law, and that, as both died gradually away, the penetration of the radiance of the sun grew toward its full unobstructed power. If the joint effects of these in the earliest ages of growth were prohibitory, they gradually gave place to those that were permissory.

The inherent influences of the atmosphere of the early stages of growth were doubtless more comparable to those of Tibetan and Titicacan regions today than to those of tropical lowlands. The atmosphere was relatively thin, its pressure low, its content of moisture limited. The normal effects of these properties should, apparently, have tended toward aridity and low temperature. The atmosphere itself, however, should have been ultra-Krakatoan in its burden of planetesimal dust, and the young earth was thus blanketed against intensities of radiance from without and inequalities of radiation from within. But no amelioration of variations could probably offset the forces of atmospheric circulation; the descending currents were almost inevitably dry, and the ascending currents precipitant, after sufficient moisture had been acquired; before that, the initiation of life was inhibited. We assume, therefore, that life-genesis was conditioned by greater or lesser variations of moisture and dryness, and hence of freshness and salinity of waters, and that these were added to the variations of radiant energy. Mild variations were doubtless helpful, violent variations destructive.

The surface conditions in the growing stages should have been rather those of eolian deposition and wind driftage than those of aqueous deposition and stream erosion, but the two processes must have joined their effects so soon as water action became an effective agency. The billowy configuration of a dune surface was probably more characteristic throughout the stages of active growth than the gullies and trenches of a highly humid region; probably the former chiefly preponderated in the areas of descending air, while the latter took precedence in the tracts of ascending air and gained in general dominance as time went on. Dunes formed of planetesimal dust should, however, have been far less inhospitable to the dawning life than modern dunes of coarse, well-rolled, siliceous sand.

The growing, billowy, porous surface of the juvenile earth very definitely conditioned the early stages of the increasing hydrosphere. The first surplus of waters condensed from the growing atmosphere would obviously have been absorbed in the deep porous mantle of planetesimal dust and planetesimal residuals that had been gathered to the earth; only later, and very gradually, would the increasing waters emerge at the bottoms of the basins; only much later would great water-bodies appear. If our picture of the surface configurations is true, the basins in the billowy eolian surface were almost without number, and the first surface aspect of the hydrosphere was that of a countless multitude of pools and lakelets. From these, in time, grew lakes and seas, and at length the oceans.

The organizing processes that led up to the biologic and psychic kingdoms may quite probably have entered upon their long career as soon as the hydrosphere began to emerge at the surface, for, with the low pressure of the early atmosphere, the temperature of the water could scarcely have been prohibitory. While we may not fix, with any confidence, the time when the hydrosphere would reach this stage, we may make this stage a fair index of the time when general physical conditions were probably hospitable to life. Starting at this stage the organizing process may have run parallel with the remaining large part of the earth's growth. In view of this, the advanced state of deployment and differentiation which life presented at the time its first good record was made in the Cambrian Period is shorn of the surprising features it bore when regarded from the older point of view, for under that view the initiation of life was necessarily delayed until after the whole gaseous spheroid had condensed into the molten globe and the molten globe had cooled to a suitable temperature.[1]

INITIAL BIOLOGIC HABITAT

It has been the habit of geologists and biologists alike to think of the ocean as the probable habitat of the earliest forms of life, and not unnaturally so; the larger part of the imperfect record of early life was preserved in marine deposits.[2] It was further noted that there were evolutions of marine forms into terrestrial forms; there were, to be sure, evolutions of land forms into marine forms, but these were easily interpreted as later reactions. An oceanic genesis of life was favored by the inherited theory of the preponderance, if not the universality, of the primitive ocean. The dominance of the primitive ocean was, however, essentially a cosmogonic deduction. The assumption of even the existence of a vast oceanic envelope, at the time life appeared on the earth, has, indeed, little other basis than is derived from some theory of the earth's origin. The oceanic view of the origin of life is therefore, at bottom, little more than a cosmogonic assumption. It is, to be sure, largely a subconscious cosmogonic assumption, but the fact that it is so largely subconscious does not add a whit to its cogency. The assumption that anything which can properly be called an ocean existed at the time life was initiated is necessarily challenged in any critical inquiry that scrutinizes the ground of each postulate, conscious or subconscious. The initiation of life required, indeed, at least the beginnings of a hydrosphere, but the presence of an ocean needs either logical or evidential support. It may, of course, be that the first favorable conditions for life were not attended by the generation of the first forms of life, and that the initiation of life was delayed until an ocean was evolved, but if the generation of life was a naturalistic process, it is theoretically obligatory on the advocate of delay to show why the process should not have proceeded when the essential conditions were fulfilled. Chemico-physical reactions usually take place when the elements are brought together under suitable conditions. The record also has its suggestions. The time required for so great evolutionary work as is implied by the wide deployment of life in Cambrian times is such as to tax even the highest probabilities of the available lapse of time. The exigencies of time render the introduction of life as early as the requisite conditions permitted, extremely probable. There appear to be no inherent grounds for hypothetically delaying the initiation of life till the oceans grew to be vast and saline.[3]

In the growth of the juvenile earth, as already noted, the first waters should have condensed from water-vapor held in the primitive atmosphere, the infantile hydrosphere growing very gradually from the infantile atmosphere. Obviously the first waters would have been absorbed into the deep porous layer enveloping the earth-body and would have crept up only gradually to the bottoms of the hollows that accidented the surface, and so, as already noted, the first appearance of the hydrosphere at the surface should have taken the form of innumerable pools or lakelets, more or less promiscuously scattered over the surface of the juvenile earth. The question of the probable bio-genetic habitat, under the planetesimal view, seems thus narrowed to an alternative between the pools and the well-watered lands, or else the shorelines between these.

What appear to have been the special demands of the case?

1. *An adequate supply of radiant energy and an adequate protection against the destructive effects of radiant energy.*—These could probably be furnished by selected depths in either the surface soils of the lands or in the open waters of the pools and lakelets.

2. *An adequate supply of carbon, oxygen, nitrogen, carbon dioxide, water, and combinations of these, together with lesser quantities of phosphorous, sulphur, potash, and other alkalies and alkaline earths, and various earthy substances.*—All these could best be supplied by the soil waters, but could doubtless be found in the waters of the pools and the lakelets. The soils were of course then purely inorganic.

3. *An adequate mechanism for holding, protecting, and preserving the products of each synthetic step in such a way as to favor the next synthetic step.*—A continuous series of such well-conserved inheritances seems a critical prerequisite to the long succession of steps involved in the complete evolution. The pores of the soil seem better suited to this than the free waters. The effects of open agitated water should have been diffusive and dispersive. The pores of the soil probably only furnished a first aid; a more specific mode of inclosure, protection, and promotion perhaps grew out of this, as noted below.

4. *A circulatory mechanism to bring suitable supplies for the new combinations, to concentrate these supplies in proper measure, and to carry away the unused and perhaps deleterious portion.*—These are functions which must be subserved in some way by the mechanism of every living organism. After such organisms had once come into being, they, of themselves, developed anatomical devices that fulfilled these essential requirements, but in the long generative process preceding the acquisition of such powers, some crude substitute for such a mechanism, incidentally furnished by the inorganic environment, was probably prerequisite to the

completion of the ascensive chain. Such a mechanism, crudely serviceable for the time, was perhaps found in the normal circulation that prevails within soils suitably situated.

A multitude of special situations were presented, but perhaps those that best fulfilled the requirements were found in the soils of the shore tracts that girt the pools and lakelets, or in the soils of the forelands that lay between these and the uplands.[4] (We use the term soil of course in its inorganic sense.) In these foreland and shoreland soils there should have been an ample supply of water while, at the same time, the fluctuations of the water-level should have been limited wherever the controlling water-level of the adjacent pools or lakelets was regulated by adequate outlets. The drainage from the uplands, passing slowly beneath the forelands and shorelands, should have given an adequate and perennial supply of all the solutions requisite for the successive synthetic combinations; while the sloping surfaces should have given adequate super-drainage and the requisite aëration. Capillary action should have drawn graduated supplies of water, carrying solutions, emulsions, and suspensions of various concentrations, up to heights in the soil which varied with conditions, while the larger pores and the upper levels were filled with air communicating with the open atmosphere. In a word, there was a reticulation of capillary water reaching up with diminishing branches, from the water-level below, and a reticulation of air reaching down, with attenuating branches, from the atmosphere above, the two interlocking in a most intricate way. Each of these advanced and retreated, reciprocally, as times of wetness and times of dryness came and went, but the mutual action of the two maintained a graded intermingling of air and water that, in later ages, has been of the utmost importance to soil-life, and was perhaps of equal importance to its initiation. There should have been intermittent water supplies from the clouds, and constant supplies by capillary action from below, while soil breathing, actuated by the pulsations of the atmosphere, should have been an effective agency in promoting a steady and controlled evaporation from the surfaces of the interlocking network of water. Evaporation above and capillary supply below, varied by precipitation, should have promoted a gentle, but effectual and perpetual, circulation conducive both to concentrations and to re-solutions, in turn, as wetness and dryness alternated. The analogy of such a soil circulation to the circulatory system of plants is very close, and the one may well have been ancestor to the other. Such a circulatory system was inevitable in the early soils under the general conditions postulated.

Obviously open waters, while not without their system of surface exchange with the atmosphere, present no interstitial circulatory system which rivals that of the soils.

5. *A supplementary mechanism by which osmotic action would be called into play with increase of efficiency in the concentration and combination of the requisite constituents, while at the same time cell-like inclosure of the synthetic compounds progressively formed would keep them in close working relations.*

While almost all substances are susceptible of taking on the colloidal state, it is actually assumed about in proportion to the complexity of the chemical composition. In compliance with this general rule, the chain of ascending carbon compounds that led up to the organic type seem to have been attended by a parallel increase in the proportion of colloids to crystalloids. At any rate, the colloids are highly preponderant in the tissues of living beings. The fluent forms, the pliancy, and the easy assumption of membranous shapes, so characteristic of colloids, as notably fitted these to take on structures of the organic type, as the angular, rigid forms of crystalloids unfitted them for like use. The suggestion at once follows that the progressive development and concentration of colloids may have been as indispensable to progress toward the formation of organic structures, as was complex chemical combination. The singular distributions and adjustments of energy in the colloid state, so far as developed in the lower carbon compounds, may have been an essential aid to the next higher steps in chemical complexity. This at least may serve as a suggestion of a possible line of evolution.

Even in the inorganic world, certain of the carbon compounds are prone to assume the colloid state. Adequate supplies of hydrocarbons and other simple carbon compounds that habitually take on the colloidal state could scarcely have been wanting in the juvenile earth. In addition to general sources, there was perhaps a special source of peculiar adaptability to the process now under study. The carbon of the sun in passing from its intensely heated state into the nebulous condition would not improbably take, in part at least, the form of carbides analogous to those found in meteorites. These, in coming into contact with air and moisture, would become reactive and form carbon compounds of higher complexity, some of which are prone to take on the colloidal state. It is not improbable, therefore, that the accessions of planetesimals brought carbides, as well as nitrides, chlorides, silicides, sulphides, and phosphides, into the growing earth-mantle, and that the spontaneous reactions of these supplied the ground-waters with carbon compounds of growing complexity and increased colloidality. These, as fast as formed, would be floated to the water surface and so enter the capillary circulation of the soils and be susceptible of such further combinations as might await them there.

The interlocking reticulum of capillary waters and air ducts in the soils needs now to be pictured a little more closely. In the layers of most effective intermingling of air and water, water films surround each soil grain. These grow thicker or thinner, or dry up entirely, as changes from humidity to aridity, or the reverse, take place. The sharper angles between grains, the smaller pores, and the narrower passages between the larger inter-grain spaces are normally filled with capillary waters likewise subject to come and go with increasing wetness and dryness. The larger pores and passageways, and inter-grain spaces, are normally filled with air. These also advance or withdraw, grow or shrink, as the state of wetness and dryness varies. The whole forms an intricate plexus of water films and capillary concentrations of water, interpenetrated with air ducts. This picture, drawn from modern soils, may be transferred, with some increase of sharpness, to the soils of the growing earth, free from rootlets and minute organisms, as well as their relics, and their interferences. Our interest centers on the progressive concentrations and deposits that would take place in the passageways and intergranular cavities in the upper soils as the result of continued capillary supply and continued evaporation in situations where leaching was unable to remove these concentrates and deposits, as must have been the case in a multitude of places in regions prone to aridity. As the soil waters were progressively evaporated, the solutions, emulsions, and suspensions borne by them were concentrated and, in part, deposited as solid films, threads, or lumps in which colloids and crystalloids must have mingled, much as they were mingled in the waters. As wet and dry seasons came and went, there must have been alternations of deposit and partial re-solutions, with the general result that, in some situations at least, the plexus of films grew thicker and more membrane-like, the pores and passageways were more and more closed, concentrated solutions and emulsions were more and more liable to be entrapped and isolated in the inter-grain spaces, and, when so entrapped, to be completely inclosed by films formed over their own exposed surfaces, thus tending more and more to convert the open reticulum into a cellular reticulum. In many a case, no doubt, the process went on until the whole mass of soil was cemented into a continuous solid by the asphaltic residue of the hydrocarbons, and the precipitated salts, or by mixtures of these; but before these ultra-results were reached, there would probably be much closure of constricted passages between larger spaces by films formed from the concentrates and so a cellular structure should not only generally precede complete cementation, but should develop widely where the latter extreme was never reached. In the course of this progressive growth of enwrapping films and closure of connecting passages, soil waters, approaching saturation, entrapped in cavities by films formed over their exposed surfaces, constituted the cell-filling and were subject to further concentration and combination. It seems inevitable that within the soils of the land surface subject to such conditions there should have grown up, thus inorganically and inevitably, not only thousands and millions, but billions and trillions of crude capsules which thus came to serve a new function in the synthetic process.

Obviously the colloids were best fitted by nature to form films and membranes. Probably the crystalloids were, on the whole, more subject to re-solution and removal, giving various degrees of porosity to the residual membranes. Various degrees of imperviousness and porosity may therefore be assigned to the crude membranes so formed.

Now, as soon as concentrates of colloids and crystalloids were thus inclosed, cell-fashion, in more or less porous membranes, the mechanism for osmotic action was provided. When the wet season returned, osmotic action should ensue. The colloids within the inclosure should be retained by the inclosing membranous walls, while crystalloids, so far as they passed into solution, might traverse these walls in either direction, as the state of concentration might require. Thus a new agency, peculiarly fitted for colloidal concentration, as well as a special means of colloidal retention, should have been brought into action. Conjointly, these qualities might apparently contribute very essentially to the further progress of synthesis.

The new agencies thus instituted would bear a very close analogy to the function of cell-inclosure and osmosis that forms the working mechanism of plants.

With the development of this quasi-cellular structure and this initiation of osmotic action between a colloid content within and the soil-solutions without, the limits to which the premises of the geologist warrant him in going seem to have been reached. The further solution of the problem of synthetic ascent seems to fall into the fields of the organic chemist, of the colloid chemico-physicist, of the biologist, and of the psychologist.

It need only be further remarked here that erosions, transportations, and redepositions, actuated by wind and water, working on surfaces devoid of vegetal protection, should, at frequent intervals, have displaced, dispersed, and replanted a part of the quasi-cellular aggregates, and thus have seeded, as it were, a multitude of new situations with new possibilities, favorable and unfavorable for further progress. The pools and the lakelets should have been thus implanted, so that if they offered conditions favorable for the further progress of the synthetic process, the advantage would have been embraced.

A distinguishing characteristic of the organic mechanism is its singular adaptation to mild temperatures and gentle reactions, and its singular efficiency under these conditions. The living body, whether plant or animal, is a mild-temperature engine. It eschews intensities and wide fluctuations. Its best work is done under equable conditions or mild oscillations. This gives strength to the presumption that life came into function under conditions as similar as might be to those equable and gentle oscillatory states of earth, air, water, and insolation, that have ever since contributed to its best activities. Perhaps there is no fact in the earth's career more remarkable than the fidelity with which the very narrow ranges of temperature, and the not less narrow ranges of atmospheric constituents essential to the evolution of life, have been maintained, while oscillations within these permissible ranges have freely prevailed. These limits and these oscillations were perhaps as imperative for life's origin as for its prolonged maintenance.

Perhaps the supreme criterion to which a hypothesis of the genesis of the earth, of the mode of its growth, and of the evolution of its inhabitants can be submitted—next after its complete fulfilment of the specific requirement of the historic vestiges embodied in itself and in its ongoings—is the fitness and the adequacy of its postulates for the task of maintaining, throughout all the earth's adolescent and adult stages, those delicate conditions that have made possible the long sequence of life and its wonderful ascent. The fidelity with which life has been furnished a suitable environment for the uninterrupted pursuit of its ascensive career, and the unbroken continuity with which the requisite sources of supply have been maintained, may well be regarded as the profoundest expression of the law of equilibrium manifested in the long course of the earth's history.

It is our personal view that what we conveniently regard as merely material is at the same time spiritual, that what we try to reduce to the mechanistic is at the same time volitional, but whether this be so or not, the emergence of what we call the living from the inorganic, and the emergence of what we call the psychic from the physiologic, were at once the transcendent and the transcendental features of the earth's evolution.

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